

A treatise on the fevers of Jamaica : with some observations on the intermitting fever of America, and an appendix, containing some hints on the means of preserving the health of soldiers in hot climates / by Robert Jackson, M.D.

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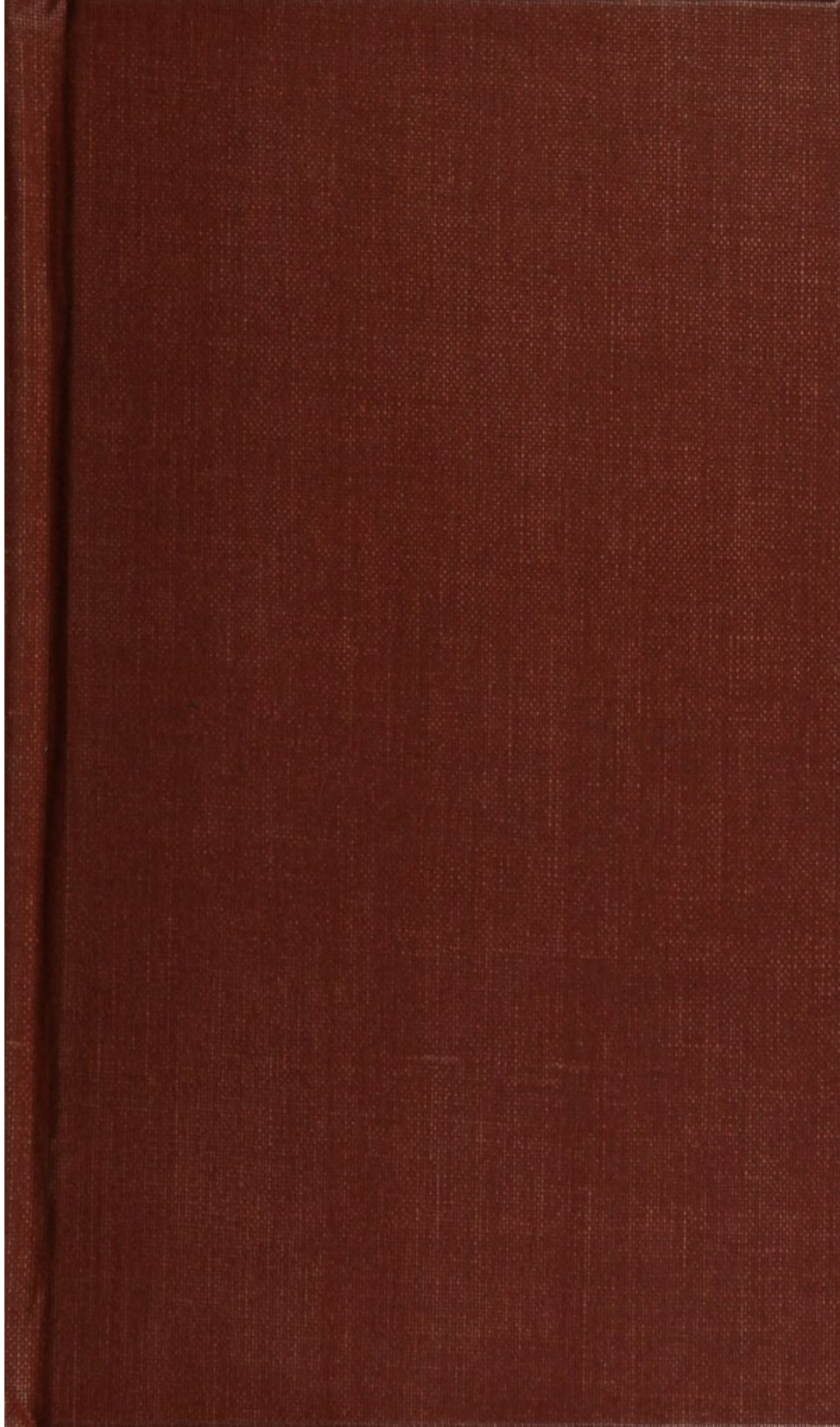
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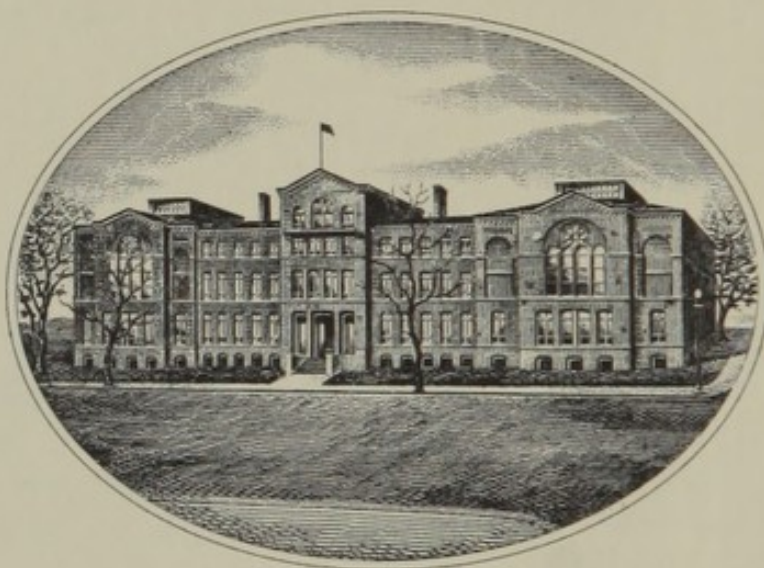
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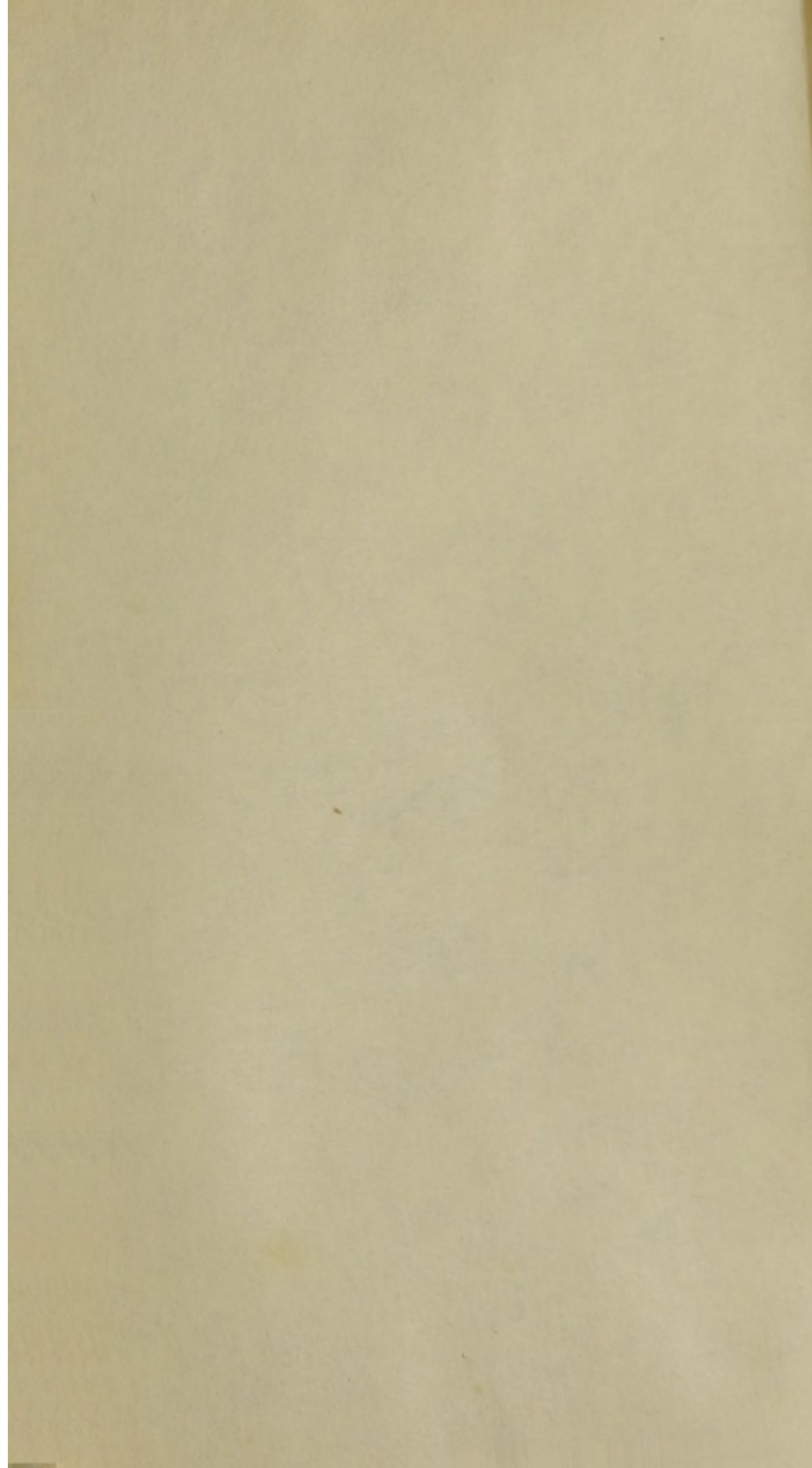
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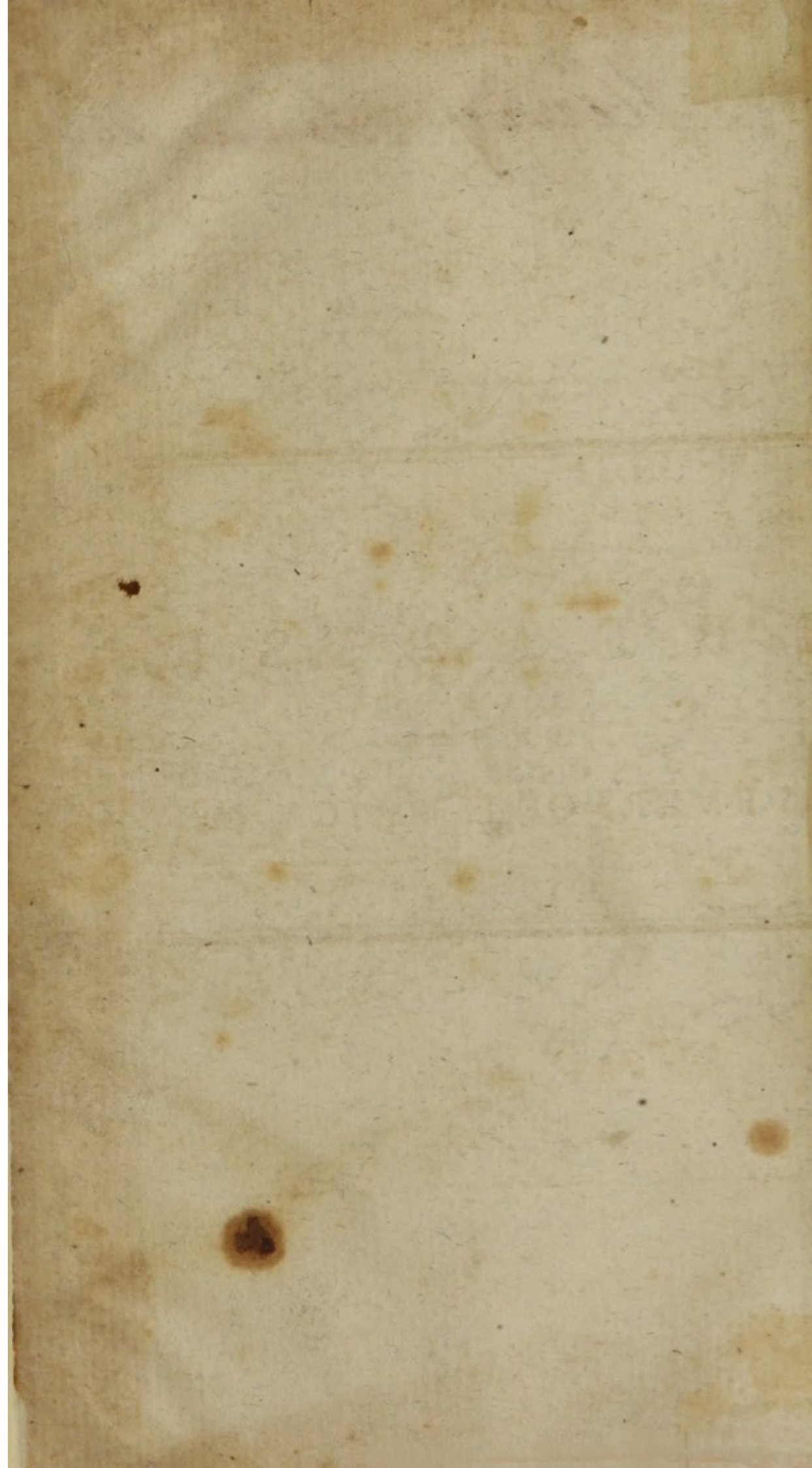
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U. S. Department of Health, Education, and Welfare
Public Health Service





Henry Brown.



A
T R E A T I S E,
O N T H E
F E V E R S O F J A M A I C A, &c.

T H E A T T E

OF THE

FEVER OF JAMAICA

BY

JOHN A. HARRIS

Lecturing Fever of Jamaica

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ON THE

Intermitting Fever of America,

AND AN

A P P E N D I X,

CONTAINING SOME HINTS ON THE MEANS OF PRESERVING THE HEALTH OF SOLDIERS IN HOT CLIMATES.

By ROBERT JACKSON, M.D.

*Ne mea dona tibi studio disposita fidei,
Intellecta prius quam sint, contempta relinquant. Lucret. lib. 1.*

—διαπειρατοι
Βροτων ελεγχος. Pind: Olym. 4.

PHILADELPHIA:

PRINTED FOR ROBERT CAMPBELL, BOOKSELLER.

1795.

Febrero

P R E F A C E.

THE observations contained in the following pages, were made during the time that I lived in Jamaica, or while I attended some part of the army in America. The materials were collected between the years 1774 and 1782; and the present performance would have been offered to the public before this time, had I sooner found leisure to attend to the business of publication. The insufficiency of Dr. Hillary's work, the most esteemed book on the diseases of the West Indies, and the only one with

which I was acquainted while I remained in that country, furnished me with a motive for the undertaking ; a motive, which may be thought, perhaps, no longer to exist, as two treatises have been published lately by Dr. Hunter and Dr. Moseley, expressly on the diseases of which I have written. I might remark, that the present attempt, such as it is, was nearly completed before the treatises to which I allude came to my hands. The views which they afford of fevers, as they differ from each other, so they likewise differ from those which I have ventured to advance. I have weighed their merits maturely, and cannot say that I discover any information, which gives me cause to change those opinions which I had formed, or which renders the publication of the present work

unnecessary. I may observe, that Dr. Hunter details, with candour and perspicuity, the mode of practice, which was followed by the most respectable medical people of Jamaica, at the time that I lived in the island. He perhaps prescribes the bark in larger quantities, than was then customary; but I do not perceive any thing in the plan of treatment essentially new; neither will Dr. Moseley, though he pursues innovation with great eagerness, be better able to establish his claim to original discoveries. The plentiful and long continued purging, on which he places a considerable share of his merit, has been a favourite practice with numbers for many years past; and the free use of the lancet, which he recommends so much in fevers, was employed in several districts of

Jamaica, before this author's name was known. Dr. Spence, a practitioner of some eminence at Lucea, in the Western extremity of the island, wrote a pamphlet (I believe in the year 1776) with a view to enforce its safety and utility, in promoting the cure of the general class of febrile diseases. The publication was well received, and served to remove the dread of the lancet, which some people till then had falsely entertained.

I have thus explained the motive which induced me to write ; and I leave it to people of experience to judge of the manner in which I have executed the task. I shall only observe, that, when I first intended to prepare my observations for the inspection of the public, I had no other design than to mention

such circumstances in the history and cure of fevers, as did not appear to be generally known. Having spent the earlier part of my life in situations, which did not admit of a continued plan of study, I had not till lately much acquaintance with the opinions of medical writers. About four years ago, when I found a settled abode, I began occasionally to look into the works of the ancient physicians. In examining Hippocrates, I discovered such a similarity in the fevers of the Archipelago, with those of Jamaica, that I suspended my design of publishing, till I should have given that author's writings a careful perusal. I soon was convinced that many observations, which I had considered as my own, were actually known to this father of physic ; and though I

was probably disappointed in being anticipated in offering something new to the public, I was still gratified by the coincidence of remark, that gave me a confidence in my accuracy which otherwise I durst not have assumed. After I had carefully perused the writings of Hippocrates, I consulted and compared such other of the Greek physicians, as I was able to procure. I even descended with a similar examination to the present times ; but as my collection of medical writers, particularly of modern ones, is small, I have probably omitted some who ought to have been mentioned, or perhaps advanced observations as my own, which in reality belong to others. If I have done so, I must be allowed to say, that I have done it without consciousness.

It may appear, perhaps, that I have treated the opinions of great names with too little respect; but if facts have at any time occurred to me, which contradict established theories, I should hope that these facts will be examined before they are rejected. No medical authority ought to prevail over the certain evidence of truth. I am not conscious of having misrepresented, for the sake of a theory, the minutest circumstance of what I have actually seen;—if I have been mistaken in any instance in forming conclusions, I shall receive the correction of my inaccuracies with gratitude.

CHAP. I.

OF THE GENERAL CHARACTER OF THE REMITTING FEVER OF JAMAICA.

AS it is perfectly well known, that fevers, which are essentially in themselves the same disease, vary in their appearances, from difference of climate and season, it would be very superfluous to attempt any proof of what is so generally acknowledged. Even Hippocrates, who lived more than two thousand years ago, does not seem to have been unacquainted with this fact. This industrious physician, as we learn from many passages in his works, not only visited the various islands in the *Ægean Sea*; but travelled likewise into various parts of the adjacent continents. The motive of his journeys, we are taught to believe, was principally to observe the different situation of places, and to mark their corresponding diseases. He has described very fully, in a curious and useful treatise, the effects of air and local situation on the human frame; and in the history of fevers, related in the books of *Epidemics*, has seldom forgotten to mention, not only the general constitution of the seasons with respect to diseases; but likewise the nature of the climate, where his observations were more particularly made. The species of fevers, it is true, that are found in the writings of Hippocrates are extremely multiplied; yet this author seems still to have been clearly of opinion, that diseases, which are essentially the same, assume, in some respects, a different appearance in the island of *Thasus*, and at *Abdera*, on the contiguous

coast of Thrace. Observations to the same effect have been made by many others in different parts of the world; nor is the fact capable of being better illustrated in any country than in Jamaica; where a very small change of place, or the ordinary revolution of seasons, are often observed to influence in a high degree, the appearances of the common endemic of that island. But this observation,—that local situation and the change of seasons, are the cause of varieties in the appearances of the same disease, has been so long known, that it is almost unnecessary to mention it; nor would it have been repeated now; unless to obviate any objections which might be made to the history, that is given in the following pages, should it not be found exactly to correspond, with that which has been observed by others, who have lived in the other islands of the West Indies, or in other parts of Jamaica; or perhaps even in the same part of Jamaica, at a different period of time. It is presumed, indeed, that the same fundamental distinctions of fever obtain in every island within the tropics; yet the description that is given in this place, (it may not be unnecessary to mention), is affirmed to be strictly exact, only in the district about Savanna la Mar; and that only for a short space of time: viz. from the year 1774 to the year 1778.

The fever, that chiefly prevailed at Savanna la Mar during the period mentioned above, was usually mild in its symptoms, and more regularly remitting in its form than I understood the endemic disease to be in most other parts of the island. To what circumstances in the local situation this might be owing, I do not pretend to determine. Savanna la Mar is situated close by the sea: its particular site, and the surrounding country to the distance of several miles is perfectly level; whilst in consequence of its being open to the sea on the east, it is visited early, and constantly by a salutary and refreshing breeze. There

is a small rivulet, indeed, which, losing itself almost entirely in mud, forms a morass that partly surrounds it on the north. From vicinity to this morass the situation of Savanna la Mar has been suspected to be unhealthy; but it is not so in fact. The sea at high water, particularly in the spring tides, overflowing the swampy ground, contributes in a great measure, perhaps, to diminish the more usual noxious qualities of the marsh exhalation. The opinion of many eminent writers, however, is not altogether favourable to this idea; but there seems to be reason to doubt, whether the opinion formed by those writers in this instance, is the result of accurate and careful observation; or if it is merely a suggestion of theory. I am inclined to the latter way of thinking, as I have never found the neighbourhood of Salt-marshes, in the different parts of America that I have had the opportunity of visiting, less healthful than the rest of the country:—on the contrary, they were frequently more so.

Before proceeding to a particular description of the fever, that chiefly prevailed in the district of Savanna la Mar, it may not be superfluous to give the outline of its character, that we may be the better enabled to trace its resemblance with the fevers of other climates, and to determine its place in the general class of febrile diseases. The common fever of Savanna la Mar, which, as I mentioned before, was usually mild in its symptoms, and regular in its form, seems to be properly ranked with those, that strictly speaking are called remitting. It is difficult to fix the boundaries between remittents and intermittents, if a fixed boundary actually exists. The paroxysms of the fever of Jamaica are observed, in many instances, to terminate in more perfect remissions than the paroxysms of the endemic of North America, which is known to be fundamentally an intermitting disease. Hence authors generally are of opinion, that all the difference which appears to take place in those fevers, depends

merely on an accidental cause: viz. on the greater or less heat of the climate. I shall not be positive that it is not so; yet I cannot help remarking that certain appearances incline me to be of opinion, that there subsists, between the endemic of Jamaica and the endemic of North America, a difference, in some degree, fixed and essential. I cannot pretend to ascertain the difference precisely; or to offer a conjecture about the modification of the morbid cause in which it consists; yet the following circumstances give reason to believe, that to a certain degree at least it actually takes place. The common fever of Jamaica, for instance, was not only disposed to terminate of its own accord; but it was disposed to terminate on certain critical days, often at an early period, and by signs of crisis too clear to be mistaken; neither did the Peruvian bark, in the manner at least in which it was managed, ever cut short its course with certainty. The endemic of America on the contrary often lasted long. It frequently, indeed, changed to another disease after a length of time; but no period could be assigned for its natural termination. The signs of crisis, it may likewise be remarked were so obscure as scarcely to be distinguished with the closest attention; at least for my own part I will own, that after an experience of several years, and the greatest care in observing the minutest circumstances, I never yet was able to say with confidence, that the endemic of America, particularly in the northern provinces, was gone not to return again, till the hour of its return was past: neither did the Peruvian bark, though its effects were so equivocal in the fever of Jamaica, scarcely ever fail of stopping the progress of this disease. To which we may add, that the complaint, which strictly speaking is called the intermittent, or ague and fever, can scarcely be said to belong to Jamaica; at least it was not known at Savanna la Mar. In the course of four years I did not once observe it; and those, who had lived much longer at this place, assured

me they had never seen an instance of it; unless in persons who were newly arrived from aguish countries. This is a fact of some importance, as it shews to us, that though the proper intermittent is not the endemic disease of the country it is still capable of existing in the climate:—no weak argument, that, the two diseases of which we have been speaking, actually do possess some difference in the modification of the general cause, though we are unable to define the precise bounds and limits of it.

The circumstances which I have mentioned might incline us to be of opinion, that the fever of Jamaica, and the common endemic of America are not exactly the same disease; that is, that though they depend on the same general cause; yet that this cause undergoes some fixed and permanent modification in those different climates, so that an essential difference actually arises. But though this really appears to be the case; yet I must acknowledge, that the reigning epidemic of the southern provinces of America, often loses its distinctive marks of intermission in the hot months of summer; whilst it approaches, in other respects, so near to the fever of Jamaica, as to be distinguished from it with difficulty. This was particularly the case at Ebenezer in Georgia, in the year 1779, during the months of June and July. A cold fit was seldom observed in this place; unless perhaps in the first attack; lowness, languor, head-ach, pain of the back and other disagreeable feelings remained even in the most perfect remissions: the disease was likewise much disposed to terminate of its own accord: on the usual critical days, frequently by signs of crisis, that were far from being obscure: yet though these resemblances were so very striking, the fever of Ebenezer was perfectly under the controul of Peruvian bark, which was not exactly the case with that of Jamaica. I must remark, however, that bark was used with a freedom in America, that I never thought of attempting in the West Indies.

But though it still may be thought doubtful by some, whether the autumnal fever of aguish countries, and the endemic fever of Jamaica are characteristically different, or essentially the same disease; yet it is not so difficult to trace a specific resemblance, between this fever of the West Indies, and the prevailing endemic of the *Ægean Sea*. The fever described in the *Epidemics* of Hippocrates has every striking feature of the disease which is the subject of the following treatise. The general character is the same; the course and mode of termination are often alike. If the duration is sometimes different, it is probably much owing to the more decisive practice of the moderns: for though it remains uncertain, whether a remedy has yet been discovered, which absolutely cuts short the fever of Jamaica in the midst of its course; yet no doubt remains, that there are various modes of treatment, which may, and actually do render the ordinary changes of the critical day, decidedly critical. The fevers of the *Ægean Sea*, as described by Hippocrates, and of Minorca, as described by the accurate Cleghorn, bear the nearest resemblance to the endemic of *Savanna la Mar*. The fevers of Italy, of different parts of the continent of Asia, as described by various writers, as well as the fever of America, of which I have had personal experience, however obscure their remissions, seem rather to be degenerated intermittents, than the disease described in the following pages. But that I may not be thought to insist too much on this opinion, which few, perhaps, may consider as of much consequence, I shall relate the history of the fever of *Savanna la Mar*, as accurately as I can, leaving it to others to determine, whether it should be considered as an intermittent degenerated into a remitting form, in consequence of the greater heat of the climate, or allowed to possess something characteristic in its own nature.

C H A P. II.

OF THE DIFFERENT TYPES OF PERIODICAL FEVERS.

A Knowledge of the types and periods of fevers, though so necessary to be well understood by those who pretend to cure the disease, has unfortunately been little attended to by the practitioners of Jamaica. In that country, I met with some who were able to foretel the returns of the single tertian; but I met with none, and I have good reason to believe that there actually were few, who troubled themselves about forms of greater complication. There were many, on the contrary, who, blinded by theories of accumulated bile, ridiculed altogether the idea of this stated regularity in the movements of nature. To enter into a dispute with such would be labour lost. The existence of a regular type in the fevers of the West Indies does not admit of a doubt, yet we must not forget to mention, that those types are traced with greater difficulty in that country, where remissions are obscure, than in others, where every paroxysm is ushered in by a cold fit. In Jamaica it is impossible to follow them without the most careful attention, or perhaps to attain a clear and systematic knowledge of them, without writing down and analyzing many of those cases which occur in practice. I remember to have been impressed with the idea, at a very early period, that one observation made by myself would in reality be more useful than twenty equally important in themselves, which I only retained in my memory from reading. Convinced of this truth I ceased to look for information in books, from the time that I arrived in the West Indies; but, trusting wholly to

my own experience, wrote down minutely, in the presence of the sick, the history and cure of the most important cases of fever that occurred to me. At stated times I reviewed that which I had done, and arranged under proper heads the most striking circumstances, that I found recorded in my notes. Among other unexpected appearances I observed a regularity and order in the types of fevers, of which at first I had no idea. But though the laws of nature appeared to be fixed and stable, in producing this varied but stated regularity of form; yet a knowledge of those laws was not easily attained. Two years were spent, and not fewer than a hundred cases were analyzed, before my views of the subject were in any degree accurate. The labour, perhaps, was superfluous; there being many authors who have described minutely every variety and every combination of type that has ever been observed to take place. But as I had little knowledge of the writings of others at this period, I shall content myself in the first place with relating the history of types as they occurred to my own observation; at the same time that I shall not omit to take notice occasionally of the more constant peculiarities, which are found in authors of credit, who have practised in different countries. The influence of climate I may observe is of considerable effect in modifying the various forms.

The single tertian, the period of which is forty-eight hours, is a form of fever that occurred frequently in Jamaica, particularly in the dry and healthy season. Its course was easily traced, as the remissions were often distinct, and the accessions sometimes distinguished by a slight horror or shivering.

But though the above-mentioned type was by no means of rare occurrence at Savanna la Mar; yet the double tertian, with similar paroxysms on alternate days, was still more common, particularly in the rainy and sickly months. This form of fever, which seemed to consist of two single tertians, that ran a separate

and independent course, began usually in the morning. Its hour of invasion was from eight to ten; and its accession was generally distinguished by a cold fit. The paroxysm, which for the most part was regularly formed, declined after a continuance of eight or ten hours; and the patient remained free from fever, not only during the night, but during the following day, till four in the afternoon, or later. A feverish indisposition usually came on then, which continued the whole or the greatest part of the night. A remission took place; but it was soon succeeded by a paroxysm similar in its symptoms, and manner of attack, to the paroxysm of the first day. This having declined towards evening, the patient, as formerly, was free from fever during the night and the day following. At the usual hour, however, or more generally before it, a paroxysm more distinctly formed in all its parts, and more violent in degree, than the preceding one which corresponded with it, returned, and continued till morning. It subsided; and was soon succeeded by the paroxysm of the fifth day, which declined, as formerly, after the usual duration. Such was the general course and progress of the disease.—The fever of the odd day, which began for the most part in the morning, usually returned later and later, and with decreasing violence every succeeding paroxysm; whilst the fever of the even day, which generally began in the afternoon or evening, as usually returned earlier, and when this was the case, frequently increased in force. The fever which came on in the morning generally began the disease. It was for the most part a fever of complete and regular paroxysms from the first attack. The fever of the evening, on the contrary, was little more than an indisposition in its beginning; nor was its time of appearing at all certain. It often was not perceived till the evening of the fourth day; sometimes not till later, neither was its continuance of a fixed duration. Sometimes it went on after the fever

of the morning had ceased; and in other cases it terminated, while the other pursued an uninterrupted course.

The type, which was so frequent in Thafus, and the other islands of the *Ægean* Sea, seems to be a species of the double tertian. Mention is made frequently of such a form in the *Epidemics* of Hippocrates; yet the double tertian of Hippocrates is directly opposite, in some respects, to the type which I have just now described.—The evening fever generally began the disease in those islands: hence the great exacerbations, and consequently the crisis were frequently on the even days. Such also appears to have been sometimes the case in Minorca: yet Cleghorn acknowledges, that a type, similar to that which I have described as prevailing so commonly at Savanna la Mar, was by much the most frequent form of the above-mentioned island. It was also, I may add, frequent in the southern provinces of North America, particularly in the hot months of summer and autumn.

A quotidian type has been described by almost every author, who has written on the subject of intermitting and remitting fevers: neither can it be denied, that forms of disease frequently occur, where the paroxysm returns every day at the same hour, with symptoms so nearly alike, that an ordinary observer can perceive no difference. Yet Mr. Senac, an author of considerable eminence, boldly maintains, that a real quotidian type does not exist in nature. The question, perhaps, is not easily determined. I can only mention from my own experience, that I have frequently seen fevers with quotidian exacerbations of such a kind, that there was no perceivable difference between them, either in symptoms or in manner of attack; yet I must likewise own, that these exacerbations were generally in the evenings; and that the disease was not in every respect of a distinct intermitting form.

I have now mentioned those types that are most

frequently met with in the fevers of Jamaica; yet besides these, there now and then occur others of more complicated and perplexing forms. Thus, I have sometimes seen at Savanna la Mar, a fever, which seemed to be compounded of a single tertian with a quotidian. In the space of forty-eight hours there were three separate exacerbations, two of which were exactly alike, that it was impossible to perceive a difference; whilst the whole three followed each other in such an order of time, that to a superficial observer, there appeared to be only one long paroxysm of thirty-six hours or more. An example will make it more plain. On Monday, for instance, the paroxysm of a fever was observed to begin about nine in the morning, preceded by some degree of coldness and shivering. This paroxysm was usually violent in its first attack: But declined gradually towards evening; though before it was gone off totally, another exacerbation commenced, which continued the whole of the night. This likewise abated on Tuesday morning; and the patient remained apparently free from fever till five in the afternoon or later. A paroxysm then made its appearance, similar to the paroxysm of the preceding evening. It ran over a course of similar duration, and scarcely had abated on Wednesday morning, when a paroxysm resembling that of Monday succeeded it; which, as formerly, declining towards evening, was soon followed by an exacerbation, that lasted till the morning of Thursday. On Thursday, as on Tuesday, there was no fever till late in the afternoon; when the evening exacerbation returning at the usual hour, proceeded in its usual course.

The tertian type, simple, or variously compounded, is the form of fever, which prevails most universally in all climates. The quartan in those countries where I have lived was rare; and the existence of a real quotidian, perhaps, is doubtful. To that compound form,

which I have just now described, I should be inclined to give the name of Semitertian. It is expressly the disease, which I now find has been described under this name by Hoffman; but it is more difficult to determine exactly, if it is the Hemitritæus of the ancients. The Hemitritæan form is frequently mentioned by Hippocrates; but his definition is too loose to enable us to judge precisely of its nature. It is in fact impossible to say with certainty, whether it is to a type similar to that which I have described, or to the extended and subintrant paroxysms of the double tertian, that he has applied the name. This last, indeed, seems to have been the idea of many of the ancients, particularly of Celsus and Agathinus.—The physicians of the earlier ages, were less curious in minute distinctions than their followers; and probably applied the name of Hemitritæus to those fevers, the paroxysms of which were simply of unusual duration, no less than to those that were of a complicated or compound nature. This actually appears to have been the case for a great length of time; but at last, and not long indeed before the days of Galen, the sophistical genius of the professors of medicine, which exerted itself chiefly in things of little importance, multiplied the types of fevers to an endless variety, and attempted to establish distinctions which have no existence in reality. Galen, who is copious in most things, has discussed very fully the subject of types in general, and described particularly the nature of the Semitertian at great length. But whatever credit may be otherwise due to the observations of this illustrious writer, it is evident that the description, in the present instance, is merely the result of theory. He has attempted, indeed, to illustrate his opinion by an example; but the case he has furnished us with, is constantly varying its hour of attack; and if accurately examined, appears rather to be a triple tertian, properly so called, than the form of fever that

I have described above. After the Greeks we may next take a short view of the Arabian physicians, who, as they borrowed much of their knowledge from the writings of Galen, likewise adopted his idea of the compound nature of the Semitertian. Avicenna, the most eminent among them, has defined this form of disease with a good deal of precision; but we do not find that he has added any thing very material to the opinions of his predecessors. Galen indeed, had discussed the subject so fully, that, though succeeding writers sometimes changed names, they do not seem in reality to have furnished much new observation. As we descend to less remote times, Hoffman and Cleg-horn are the most accurate of the moderns, on this subject, who have yet come to my hands. The former describes under the name of Semitertian the express form of disease, that I met with in Jamaica; the latter refers this title to the extended and subintrant paroxysms of the double tertian. It is with unwillingness that I differ in opinion from Cleghorn, who has thrown more light on the history of periodical fevers, than perhaps, all the writers of his time: yet I cannot help observing, that I never recollect to have met with an original Semitertian, such as that he has described in his well-known treatise. I must own, indeed, that I have several times seen the morning fever of the double tertian anticipate, so as to be mixed with the decline of the paroxysm of the preceding evening; thereby producing a form of disease, that could only be distinguished, by the most careful attention, from one long paroxysm of thirty hours or more: yet this was in fact, only a degenerated double tertian, the paroxysms of which became accidentally mixed with each other.

The types which I have described above are all the varieties, that I observed in the fevers of the West Indies; yet more extensive experience might have, perhaps, brought to my knowledge still further com-

plications : for besides the forms mentioned in the preceding pages, I had the opportunity, in the southern provinces of North America, to see several instances of the triple tertian, properly so called. In forty-eight hours there were three separate paroxysms ; all of them differing from each other, but corresponding with others that followed.—They were easily traced, as the accessions in that climate were usually distinguished by a cold fit. In Jamaica, on the contrary, complications and irregularities were marked with difficulty. The anticipation of type among other things occasioned considerable perplexity. Thus the single tertian, whose regular period is forty-eight hours, often completed its revolutions in forty-six. But though the paroxysms frequently returned one hour or even two hours sooner than the usual time ; yet these anticipations were seldom longer, unless the disease was of a malignant nature, or disposed to change to a continued form. In either of these cases anticipations of twelve or fourteen hours were not uncommon. Of the different forms of fever the anticipations of the single tertian were the longest and most remarkable. It was observed, indeed, that the evening paroxysm of the double tertian very generally returned before the usual hour ; yet this return was seldom more than an hour at once ; nor did the time of invasion in the whole course of the disease, so far as I have observed, ever go beyond twelve at noon. The morning paroxysm, on the contrary, frequently postponed ; yet I have likewise observed it sometimes to anticipate six or eight hours at one time ; by which means, it encroached on the paroxysm of the preceding evening, and produced the disease which Cleghorn has distinguished by the name of Semitertian. The observation of this fact does not seem to have escaped Avicenna.

There undoubtedly are accidental circumstances, which have some effect in accelerating or retarding

the return of the paroxysm for a short space of time; yet it would appear, upon the whole, that the cause, why a fever anticipates or postpones, depends upon something that is peculiar to the particular nature of the disease. Thus, a fever, which once begins to anticipate, generally goes on anticipating through a great part of its course; a certain proportion being frequently preserved between the anticipations of the different paroxysms. This was particularly the case in the single tertian of America. The paroxysms of the fevers of that country often anticipated to a certain point, by short anticipations, which bore a regular proportion to each other; whilst they were sometimes likewise observed to postpone, in the same gradual manner, towards the termination. This seldom happened in the fevers of the West Indies.

The anticipations I have mentioned often occasion perplexity in tracing the types of fevers; yet the complications which arise in the progress of the disease, have a still more considerable effect in embarrassing the appearances of regularity. Fevers which begin as single tertian, often continue such through the whole of their course; yet it sometimes likewise happens, that complicating fevers make their appearance on the even days for instance, and continue longer, or terminate sooner than the original complaint, in such manner, as if their existence no way depended on it. To be able to distinguish those complications from the anticipations of a single type is frequently very useful, and a knowledge of it may, in general, be attained with a good deal of certainty. Thus for instance, if the disease is moderate in its symptoms, and without suspicion of malignity, the appearance of a paroxysm twelve or fourteen hours before the usual time, especially if there is no material difference in the nature of the symptoms, gives reason to suspect that the premature return is in reality the complication of another fever. On the contrary, where the disease

has betrayed signs of malignity, or where the symptoms differ from those of the former paroxysms only in a greater degree of violence, there is then reason to apprehend that this appearance is only an anticipation. On the subject of complication of type, some curious observations may be found in the writings of Galen and Avicenna. The opinions of those authors, indeed, are often mixed with whimsical theories; yet in many respects they are fundamentally true and highly interesting.

It would be a matter of some utility could we learn to foretel, from the nature of the types, the accidents that are likely to happen in the progress of the disease, or to form a probable conjecture of the event. Something certainly may be gained if we observe with attention. Thus I may remark, that I never found anticipations of one hour or even two to be of much consequence in the fevers of Jamaica, particularly if they happened at an early period; yet if they were longer, or did not happen till after a long continuance of the disease, they often indicated an approaching crisis. On the contrary, where the paroxysm anticipated twelve or fourteen hours at one time, there was always suspicion of danger, at whatever period this might happen. It either indicated danger and malignity, or a disposition in the fever to change to a continued form. Anticipating fevers were likewise observed to be more disposed to terminate of themselves, and likewise to terminate more speedily, than those which steadily preserved the same hour of return. This seems to have been known to the ancients.—But farther, as anticipating types are generally a sign of increasing violence, though of a more speedy termination; so the postponing of the paroxysm, has usually been allowed to indicate a disease, whose violence has begun to decline. Such is the common observation; nor have I ever found it to be otherwise; unless in some cases of weakness and im-

paired sensibility, where the fatal paroxysm did not come on till after the usual hour of attack.

I am afraid I may appear to many as unnecessarily minute, on a subject, which is not in general considered as of much importance; yet still I cannot forbear to mention some circumstances of connection, between the hour of invasion and the future type of the fever, which appear to be not only curious, but useful; and which, so far as I know, have not been taken notice of by any preceding writer. Galen, it is true, boasts the knowledge of foretelling, from the appearance of the first paroxysm, the nature of the future type of the disease; yet the hour of invasion is not included among the number of those signs, from which he has drawn his information. The rules, indeed, which he has left us are not altogether without foundation; yet they are by no means to be depended on alone. They have, in fact, arisen from theories of bile and phlegm, rather than from accurate and careful observation. There is not, perhaps, any one criterion on this subject that applies equally in every climate. Those rules, which I thought I had discovered, are only local. They varied in the different parts of Jamaica, and in most parts of America did not apply in any degree. Having adopted, on my arrival in the West Indies, the method of writing down and analyzing such cases of fever as came under my care, the difference of hour, at which fevers of a different type usually commenced, struck me as a matter of no small curiosity. The single tertian, for instance, was constantly remarked to begin in the forenoon, usually between the hours of eight and eleven; whilst those forms of disease, that were evidently quotidian, or still more continued as constantly began in the evening, generally from four to eight. This seemed to be a fixed distinction; it is an important one; and so far there was no ambiguity: but it was extremely difficult, perhaps impossible, to

discover signs at an early period, which were capable of distinguishing the fever, which continued simple in its form throughout, or which became complicated at a certain period of its course. I have often made a fortunate conjecture; but I believe it would occasion embarrassment, rather than afford information, were I to attempt to describe those circumstances, which sometimes determined my opinion. They must, in fact, be learnt from actual observation. I have just now mentioned, that it is extremely difficult to distinguish the single from the double tertian, by the appearances of the first paroxysm, when the morning fever of this last form begins the disease; so neither is it easy to distinguish the double tertian from the quotidian, when the illness commences with the evening paroxysm of that complicated type. This is a case, indeed, as far as my experience goes, rarely happens; yet where it does happen, the circumstances, with which the evening fever is usually attended, mark a further distinction. The quotidian commences usually by a distinct and regular paroxysm; the evening fever of the double tertian, for the most part, only by a slight indisposition.

This connexion which I have just mentioned, between the hour of invasion and the type of the fever, was observed constantly at Savanna la Mar in Jamaica; but the same rules did not by any means hold true in the different parts of the continent of America. The most usual hour of the invasion of the single tertian was twelve at noon in that country; though in some cases the paroxysm came on so early as ten in the morning, or so late as two in the afternoon. Of the other forms I cannot speak with any certainty.

But besides the different hour of invasion of the different types, I must likewise take notice of some other circumstances, that seemed to be connected with the various forms. The duration of the paroxysm for instance, was usually longer in the single tertian than in the double tertian, or quotidian; and

of the double tertian, the paroxysm of the morning was usually longer than that of the evening; and besides being longer, was generally of greater violence, at least in the beginning of the disease. The cold fit was likewise more remarkable in the single tertian, than in the other forms of fever.—I do not speak of the quartan, of which I know but little. Of the bilious vomiting, so much insisted on by Galen as a distinguishing mark of the single tertian, I am at a loss to speak positively. I can, indeed, affirm that I constantly observed such evacuations to be more frequent in the different forms of the tertian, than in those that appeared to be quotidian, or that approached nearer to a continued type.

Such were the types of the fever of Jamaica, and such were the principal circumstances that appeared to be connected with them. The types of fevers it may be observed seem to be modified by climate; and there are not perhaps two different countries, or even districts of country, in which they are exactly alike. The hour of invasion of the same form of disease was different in Jamaica, and on the continent of North America; neither does it appear to have been exactly the same in Jamaica, and in the different islands of the Mediterranean. The prevalence of certain forms in certain climates, and the various changes of the form according to the changes of the season, is a matter of curiosity, and of considerable importance in the history of the disease. In Jamaica for instance, for one single tertian, there were at least three double ones; whilst in America, the single tertian bore the proportion of ten to one, perhaps, to all the other forms. In the higher latitudes of this country, the single tertian was almost the only form which was seen, in the winter months and in spring; yet in the southern provinces, particularly in the heat of summer and sometimes in autumn, the double tertian, and even still more complicated types were by no means uncommon.

I have thus endeavoured in the preceding pages to describe the types of remitting fevers, as they occurred to my own observation. I have likewise attempted to compare my own descriptions with those of other authors; so that the reader may, in some degree, be able to attain a systematic view of this species of disease, as it appears in the different parts of the world. The labour I am aware will be reckoned superfluous by many; and a discussion on types, will probably be considered as partaking too much of the school of Galen, who is held in contempt by the writers of the present day. I by no means contend for the infallibility of Galen or the ancients; yet I cannot help believing, that though they have left us much false and superfluous theory, they have likewise left us many valuable observations, on the nature and progress of febrile diseases, which the moderns seem to have neglected. Though the theory of Galen on the present subject is probably ill founded, his observations are certainly exact; and from what I have myself seen, no assertions, which will convince me, that an intimate acquaintance with the types and periods of fevers, is not an essential knowledge to the practitioner. It is, indeed, the first step; and it is a step of such importance, that our future progress will neither be satisfactory nor safe, unless it is properly understood. I may be allowed to speak from my own experience. It fell to my lot to have the charge of men's lives at an early period of life. I had then no knowledge of the types and periods of febrile diseases, and I must not conceal, that the method of treatment I pursued, and the returns of the disease so often interfered, that though actual harm was seldom done, yet many opportunities of doing good were certainly lost. The patient, if he had the least penetration, could not, sometimes, avoid seeing, that, though I might be acquainted with the common routine of practice, I was ignorant of the nature and progress of the disease, from which I had undertaken to relieve him.

C H A P. III.

CRITICAL DAYS IN FEVERS.

THE critical days, which are intimately connected with the types and periods of fevers, come properly to be considered in this place. The subject is important, and though very fully discussed by medical writers, does not as yet appear to have been satisfactorily explained by any one. If we attempt to trace the doctrine to its source, we shall find the first mention of it in the writings of Hippocrates. The followers of this author's opinions are numerous; and the endeavours, they have employed in attempting to establish his system, have been strenuous and unremitting. But critical days have unfortunately afforded a field for controversy in all ages; and there still are many who maintain, and others who as confidently deny the existence of any such power in affecting the termination of febrile diseases. In the midst of this perplexity, a man who has had no experience of his own is totally at a loss, which way to turn. There are great names on both sides of the question, but clear and decisive evidence on neither. A detail, therefore, of such observations as promise to remove many of those circumstances of embarrassment, cannot fail of being useful, and I hope acceptable to the public. I am aware, indeed, that an attempt to explain a mystery, which has hitherto eluded the researches of the most eminent physicians, will not probably escape censure:—In an obscure man, perhaps, it may be deemed arrogant; neither is it altogether without hesitation that I undertake the discussion; though very unequivocal proofs of the truth of the principles on which I proceed, arise from a view

of the cases of fever which came under my care, during the time I lived in the West Indies. The principle on which the critical days in that climate depend, appears from the facts found in the following pages to be indisputably established; the deviations are satisfactorily accounted for; and the fundamental rules, it is presumed, are such as may be extended to every climate on the globe. But, I shall state in a few words the leading circumstances, which occurred to me on the subject. If they afford not light enough to remove all the difficulties, it is hoped they may at least point out a road, by which these difficulties may in future be removed.

It may not be improper to remark in the first place, that I had heard of the doctrine of critical days in fevers before my arrival in the West Indies; yet I may likewise observe, that it was a doctrine, which I had only heard of by name. I had no knowledge of it, and I soon found that the idea was treated with ridicule by practitioners, who very generally supposed the course of the fevers of Jamaica to be cut short by bark, or other powerful means. Influenced, perhaps, by the authority of older men, I found myself disposed to acquiesce in the common opinion, that this doctrine was only one of the fanciful theories of the schools; yet it was not long before I acquired a different view of the subject. I soon observed that fevers sometimes ceased before a single grain of bark was given; sometimes after a few doses, and sometimes not till after several ounces. The observation of this fact did not fail to undeceive me. Under the circumstances I mention, it would have been the height of obstinacy to have persisted in believing, that the cure of the fever was in reality owing to the power of this celebrated remedy. But though it was soon evident, that the termination of the disease depended on some other thing than that which was generally supposed; yet a considerable time past over, before I was able to determine what this something actually

was, or before I was able to ascertain the laws which it obeyed. I soon discovered, indeed, that fevers had a general tendency to terminate on particular days; but it was not till the year 1776, that I discovered the proportion those days bore to each other, or the sources of the many deviations, which occurred. The subject seemed to be important;—and I felt an eagerness to determine a question, which hitherto had been fruitlessly pursued. With this view I wrote down with care and attention every case of fever which I met with in the year 1776 and 1777; and in looking over the memoranda sometime after, found, that the critical days bore to each other the following proportion: viz. of sixty cases, which terminated favourably, ten terminated on the third, ten on the fifth, twenty on the seventh, ten on the ninth, five on the eleventh, three on the thirteenth, and two on the seventeenth. Of nine which terminated fatally, one terminated on the sixth, one on the seventh, six on the eighth, and one on the tenth. These facts are precise and determinate; but I must not forget to mention, that if we are guided wholly by obvious appearances, there sometimes occur circumstances, which occasion embarrassment. Thus in the present instances, I not only reckoned the time by the periods, or revolutions of the disease, but I likewise simplified the complicated types; that is, I reckoned every revolution of the single tertian as forty-eight hours, though it was often completed in less; whilst I considered the corresponding paroxysms of the double tertian, only as the same disease. It may also be farther remarked, that the disease, which was the subject of this investigation, was of a regular, remitting form. Paroxysms and remissions were always discernible, and signs of crisis were generally distinct.

The state of the critical days, as represented above is literally exact, where the complicated types were simplified, and where the time was reckoned by the periods of the disease; but I must likewise observe, that

unless this method of calculation was adopted, there occurred numerous instances, which seemed to deviate from the general rule. In the first place, if the type of the fever was single tertian, which neither anticipated nor postponed,—and with paroxysms which did not exceed twelve hours in duration, the crisis was uniformly on an odd day: yet if the type anticipated, and the sum of the anticipations, in the course of the disease, was equal to twenty-four hours, the crisis was then necessarily removed to an even day, if the time was reckoned by the natural day; though still on an odd day, if reckoned in the manner which has been mentioned above. In like manner, if the type postponed, while the duration of the paroxysm exceeded or amounted to 24 hours, the crisis was necessarily protracted to an even day. But this was a case, which seldom happened. In fevers likewise of the double tertian type, the type which prevailed principally at Savanna la Mar, there occurred much seeming irregularity. This form of fever, as was said before, seemed to consist of two diseases, which ran a separate and independent course. Thus, if the fever which began on the odd day was critical; that is, if the paroxysm of the odd day terminated the disease, the crisis was necessarily on an odd day; but if that fever, the first attack of which was on the even day, consisted of an equal number of paroxysms with the other, or continued after that had ceased, the crisis was then on an even day, reckoning from the beginning of the illness, though still on an odd day, dating from the commencement of the second fever. It was the observation of this fact which first gave me the idea of simplifying complicated types, and of calculating the critical days by the periods of the disease. The idea may perhaps be reckoned fanciful; but experience has afforded me sufficient proofs, and it will still afford the same to those who take the trouble to look for them, that the various types of complicated fevers

actually run a separate and independent course; a fact when established, which removes all doubt and ambiguity from the apparently varying laws of critical days in the compound forms of febrile diseases. With regard to the quotidian it remains to be remarked, that the crisis was generally on an odd day. It was likewise generally on an odd day in those that were still more continued and acute;—a fact which seems to have been well known to Avicenna. But though the rules I have mentioned are clear and uniform, I must still own, that I have sometimes met with fevers of a very continued kind, which terminated late on the sixth, or rather very early on the seventh. The disease was then of more than usual violence on the sixth:—how far this might be owing to anticipations of the paroxysm of the seventh, accumulated upon that of the sixth, is difficult to determine with certainty.

The anticipation, the postponing, and the complication of type are the principal circumstances, which usually disturb the regular critical periods in fevers of short duration; yet in those of longer continuance, there is still another cause, which deserves to be particularly attended to. In the fevers of Jamaica, especially in those which approached to a continued form, some very apparent change in the nature of the symptoms, or in the mode of action of the febrile cause, was generally observed on the seventh, or before it. In consequence of this change, the order of the critical days was sometimes disturbed, and appearances were often produced, which seemed to contradict the rules, which we have attempted to establish. It was a common remark, that after the seventh there was less apparent regularity in the movements of nature, This, as we shall afterwards attempt to prove, was the consequence of a septenary revolution, which accidentally disturbed the regular order of the ordinary days of crisis. It is a fact of which the ancients were not ignorant; and of which I shall have occa-

sion to make frequent use: viz. that a relapse has a tendency to run over a course of duration equal to the original fever. This is confirmed by the authority of Hippocrates; but I may also add, that not only those recurrences of fever, which are more properly styled relapses; but further, that in those instances, where the disease undergoes any remarkable change in the nature of its symptoms, the disorder is generally disposed to continue for the same length of time in this new form, as it had done in the former. Thus a remarkable change of symptoms on the fifth was followed by a crisis on the ninth; sometimes, perhaps, only by another change of symptoms on the ninth, the final crisis not happening till after another period of five days. In like manner, a change of symptoms on the seventh was often followed by a crisis on the thirteenth; or only, perhaps, by another change on the thirteenth, the disease completing another revolution of seven days before a final termination. That such changes actually do take place at certain periods, not only those cases of fever, which have come under my own care, but those related by Hippocrates, in the books of Epidemics, give sufficient room to believe. Thus in every one of those instances, where the history is so circumstantially detailed as to leave it in our power to trace the disease in its progress, it will constantly be found, if the day of crisis deviates from the general rule, that a change of symptoms, often an evident renewal of fever, had actually taken place at some period of the course. In this manner, if the change of symptoms of which I speak happened on an odd day, the odd days continued to be critical, as if no change had been; on the contrary, if the paroxysm of the odd day completed its course, the remission which followed was often more perfect than usual:—a distinct period was marked in the history of the disease,—or in other words, there was an obscure or imperfect crisis. But on the

day following, which was an even day, a fever with a different train of symptoms made its appearance, and ran over a course, for the most part, equal in duration to the former. If this change, or renewal of the disease happened on the sixth, a change or crisis was not expected till the tenth, if on the eighth, not till the fourteenth. I have said just now, that relapses were generally disposed to run over a course of the same duration as the original disease; yet I must likewise remark, that they were sometimes also of shorter continuance. Thus I have frequently observed a change of the nature of the symptoms on the seventh, and a final crisis on the eleventh; the renewal of the disease, instead of seven, being only of five days continuance.

The above circumstances are capable of explaining the ordinary deviations from the regular critical periods in the fevers of the West-Indies; but I cannot affirm with the same certainty, that a similar explanation will be constantly admitted in the long fevers of this country. I have however reason to believe, that changes at the septenary periods frequently take place here, and sometimes apparently disturb the critical periods of the disease. Those cases which I have been able to trace with accuracy give strong proofs of it,—I shall relate two or three of them to serve as an illustration. The first, is that of a young man, who had been ill of a fever more than three weeks before I was called to him. Two days before I saw him; and after an evident abatement of the symptoms, there happened a sudden and unexpected relapse, or renewal of the disease. Informed of this circumstance, I dated from the new attack, and calculated the critical days in the manner which has been shewn above. Minute attention discovered the type, though it was only an obscure one.—It was Semitertian, or there was an exacerbation every evening, with a more evident paroxysm

on the alternate days. A crisis happened at the period I had foreseen, but it was not final. A fever returned again in the evening, different however in type, as well as in symptoms, from the preceding. It had distinct quotidian exacerbations and an imperfect crisis happened on the seventh. But in twelve or fourteen hours, a coldness and shivering marked a renewal of the old, or perhaps the invasion of a new disease. The symptoms were not only different in their nature from the symptoms of the former; but they were likewise more violent in degree. The disease continued in this form for seven days, and the crisis, which at last was only imperfect, was soon succeeded by another renewal of fever, the beginning of which was marked by a similar degree of coldness and shivering. The symptoms of this were likewise different from the preceding, but its form was the same, and it ran over a course of equal duration. The septenary revolutions were very plain in this case. I shall relate another in which they were not so clearly marked, though they certainly did still take place. It is a case of fever with nervous symptoms. On the seventh a sediment appeared in the urine, some drops of blood fell from the nose; and the abatement of fever was very evident; yet it did not last long. The disease recurred again on the eighth, and continued to increase in violence till the fourteenth. A sediment then appeared in the urine, some drops of blood fell from the nose as before, there were two or three evacuations by stool, which had been unusual in the preceding course of the disease; and from the whole appearances I could not help entertaining some faint hopes of crisis. There was indeed an evident alleviation of the sufferings; but it lasted but for a short time. Next day every symptom was aggravated, and the powers of life seemed to suffer a gradual diminution till the twentieth, when the patient died. I do not recollect any instance of fever, where the revolutions were more ob-

seure than in the present case; yet they were still capable of being traced. The next example I shall mention is much clearer. It is an instance of a bad fever, of no discernible type in the beginning, in a man who was considerably advanced in years. On the evening of the seventh there was some obscure tendency to crisis. The patient was not only easier in his own feelings; but the eye and countenance, which had been confused and clouded, brightened up, and a small sediment appeared in the urine. Yet these favourable circumstances were only of short duration. In the course of the day following, all the symptoms recurred, and the disease acquired force till the evening of the thirteenth. The pulse then began to rise, and continued rising till the morning of the fourteenth, when a profuse sweat was followed by a very distinct crisis. But still this crisis was not final. The malignity of the disease, however, departed, and the complaint that remained, assuming a remitting form, totally disappeared after another period of seven days. I shall only beg leave to relate another instance of fever, which occurred to me lately, and which affords a very curious proof of septenary revolutions in febrile diseases of long continuance. A young man had been ill of a fever about a fortnight before I was called to him. At the time I first saw him, the symptoms were very violent; but having abated considerably in the course of a day or two, I began to entertain hopes of a speedy recovery. The complaint was almost entirely gone, when a new train of symptoms unexpectedly making its appearance, raged with violence for a day or two, and then declined gradually as the other had done. I again looked for signs of crisis, when another accession on the seventh from the former attack, brought matters into still greater danger. These symptoms, though of a different nature from the former, were violent in the beginning; but they soon began to abate, and had

almost disappeared, when the attack was once more renewed on the following seventh. In this manner the disease went through nine septenary revolutions; and it is somewhat remarkable, that the symptoms, which marked the new accession, were always different from those of the accession immediately preceding. In one, the distinguishing symptoms were a morose and stern sullenness, in another, delirium, tremors and subfultus tendinum,—and in the third, copious liver-coloured stools. These were three times severally repeated. It deserves, however, to be remarked, that the period of the accessions was shortened before the termination of the disease. After it had continued nine weeks in the manner I have described above, there were two accessions of five days each; after which all traces of fever disappeared.

It is sufficiently plain from the facts which I have mentioned in the preceding pages, that the more usual irregularities in the order of the critical days, proceed generally from overlooking the type in periodical fevers, or from neglecting to attend to septenary, and other revolutions, in such as approach more nearly to a continued form. These are the general causes of apparent irregularity; yet besides these, there are still some others, which must not be passed over without notice, as they occasionally have the effect of producing apparent deviations. Thus it often happens, that a disease, which appears to be continued in the beginning, changes to remitting after a certain duration. The change is usually on an odd day, and on the day following the first paroxysm of the remitting form makes its appearance, the termination of which may be expected on an even day, if we date from the beginning of the illness, though still on an odd day, if we date, (as perhaps we ought to do) from the time this change in the circumstances of the disease took place. To this we may add, that those complicating fevers, which, happening at various dis-

tances of time, sometimes terminate sooner, sometimes continue longer than the original complaint, frequently disturb in appearance the general regularity of the critical periods of nature. It happens, perhaps, from a similar cause, that a paroxysm of an unusual kind sometimes terminates the disease, and apparently disturbs the regular periods of crisis. This has occurred to me several times in practice; and it happened twice in my own person. The ordinary paroxysm declined after the usual duration; a new one succeeded of uncommon violence, and very different in its nature from the former. Its course was of long continuance, and it finally terminated the disease.

The above facts enable us to explain satisfactorily every circumstance, which relates to critical days in fevers, where the crisis is clear and decided; yet I must still own, that as I have sometimes met with fevers where marks of crisis were scarcely perceptible so it would be rashness, in such cases, to speak positively of the order of the critical days. The patient might, in some measure, be said to wade through the disease; the changes from day to day being so very small, that it required more discernment than I can boast of to mark them with precision.

The observations I have related, and the rules I have attempted to establish, for the better explanation of the doctrine of critical days in fevers, were formed at a time when I had no knowledge of the opinions of preceding authors. They may therefore better claim exemption from bias in favour of one set of writers, or prejudice against another. They are indeed no more than an analysis of facts, which were collected with every possible care, which are sufficiently circumstantial, and which speak best for themselves. They contain, (if I do not view them with a partial eye,) such information, as may lead to a satisfactory explanation of this mysterious and long

disputed doctrine.—I must only beg leave to add, that though I have everywhere mentioned the pre-eminence of particular days in terminating fevers, yet it must not be understood, that this power depends on a particular quality of the days, merely as such. It depends more evidently on a certain number of revolutions of the disease, in consequence of which, the fever from something we do not in the least understand, seems disposed to terminate finally, or to suffer a change in its mode of action. This therefore brings us to the conclusion, that the critical periods are improperly calculated by the natural day. The doctrine, in short, can only be rendered consistent by attending to the periods of the disease, by simplifying complicated types, and by marking those septenary or other revolutions, which happening at different distances of time, occasion an appearance of irregularity which does not exist in reality.

Having related the result of my own observations on critical days in fevers, I shall now endeavour to bring under one point of view, the substance of what has been written on the subject, by some of the most celebrated of the ancient, as well as modern physicians. That certain days, or that portions of time comprehended in a certain number of days, had obviously a power of producing changes on the human frame, appears to be an observation of high antiquity; but as a medical doctrine, we are unable to trace it farther than the days of Hippocrates. Hippocrates has treated very fully of the critical periods of fevers, in various parts of his works; and upon the whole, has amassed a considerable body of information; though with less precision, perhaps, than has been generally imagined. The cases of the Epidemics, which we naturally consider as the materials from which he formed his general doctrine, have some obvious and great defects. The date is seldom clearly ascertained, and the mode of calculating the time, does not seem to be fixed. If

a fever, for instance, begins in the evening, or in the course of the night, the day following is generally reckoned the first day of the disease, by this author.—But this is not all.—Some of the cases are plainly related from memory; and others are only parts of cases, related by different persons. This want of accuracy, where it is scarcely possible to be too circumstantial, necessarily breeds confusion, and produces an appearance of irregularity, which does not actually exist. Hence we find inconsistency in the general doctrine, as delivered in different parts of the works, which have been ascribed to Hippocrates; at the same time, that there is a want of that circumstantial detail in the particular parts, from which only we can be enabled to form an opinion. I have read over with much attention the cases of fevers, recorded in the Epidemics; but I frequently found myself unable to trace the disease in its progress. Though evidently subject to periodical movements, it was not always in my power to lay hold of the type; yet wherever it was possible to attain this exactness, I have the satisfaction to add, that I constantly found the movements of nature to be uniform. They were the same in the islands of the Archipelago, as in the island of Jamaica.—If they appeared in some instances to be different, it was perhaps principally owing to this, that the Greek physician had left some part of the disease undescribed.

From what I have just now said, we can have no hesitation in concluding, that the opinion of Hippocrates, on the subject of critical days, is neither precise in any one part, nor consistent in the whole. The doctrine, however, in its best digested form, is the following: viz. That odd days have a remarkable power in terminating fevers; but more particularly, that the great critical revolutions happen at quaternary periods. Thus the most eminent critical days, are the fourth, the seventh, the eleventh, the four-

teenth, the seventeenth and the twentieth. This is the general form of this Hippocratic doctrine; yet in this form, it bears contradiction to observations that are found in various parts of that ancient author's works. The fifth and ninth are excluded by this arrangement, from the number of the critical days; though there are numerous examples of their great power, in terminating febrile diseases.

The doctrine of critical days, which appeared first in a regular form, in the writings of Hippocrates, found numerous and respectable advocates among the ancient physicians. Diocles of Carystus, Philotimus, Heraclides of Tarentum, &c. all bore testimony to the general truth of the observation; but their writings being unfortunately lost, we are now ignorant of the particular facts and arguments, by which they attempted to support their opinions. Indeed, from the time of the Persian invasion of Greece, till the Roman arms penetrated into Asia, a period of near four hundred years, we know of no opposition to this fundamental doctrine of the Coan Sage: But in the time of Pompey the Great, an author arose, who endeavoured to establish his own fame on the ruins of this favourite system of his predecessors. Asclepiades, who was a man of a bold and daring genius, not only rejected this apparently well founded doctrine of the ancients, but treated the idea of it with ridicule. His arguments are ingenious and acute; but they fall short of the truth. The paroxysms or exacerbations, as he justly observes, sometimes change to the even days, and consequently the crisis: yet this, if properly understood, does not destroy the generality of the rules;—if the method of calculating the time, by the periods and revolutions of the disease, be adopted, the difficulty is perfectly removed. But though this fact in reality, was not unknown to Asclepiades; yet it does not appear, that he understood the application of it. I may add, that

he has precipitately rejected the doctrine, from the very circumstance which establishes its reality.

We do not meet with any thing very material, on the present subject, between the time of Asclepiades, and the days of Galen. There appears, indeed, to have been many, who adopting the opinion, and copying the arguments of the eloquent Bethynian, denied altogether the existence of critical periods in fevers; whilst others, recurring to the doctrine of Hippocrates, maintained their reality with no less obstinacy. But we are now in a great measure ignorant, if those writers attempted to support their opinions by any new facts, or new arguments. Among other misfortunes, we must regret particularly, that the treatise of Aretæus on fevers is lost. From what we know of this author's industry we might have reasonably expected original information on the subject in question.

Galen, whose fertile and exuberant genius left no path in physic unexplored, has written fully on this celebrated doctrine. He has professedly adopted the opinion of Hippocrates, and laboured much to explain and confirm it; but unfortunately, he has oftener overwhelmed the subject with diffuse and tedious reasonings, than illustrated it by proofs from experience and actual observation. Upon the whole, however, amidst much superfluous and unmeaning matter, we find not only useful information, but a more systematic arrangement of facts, than is any where to be met with. He has attempted to fix with more precision the date of invasion; he has estimated with more accuracy the critical power of the different days; and further, has hinted obscurely, that the time will be calculated most conveniently by the paroxysms or revolutions of the disease. In short, this author, no less than Asclepiades, was sufficiently acquainted with the principal truths, which give consistency to this doctrine; but it is evident, that he

did not understand the full extent of their application. He was constantly biased by the theory of a quaternary period; as without this predilection, it is not easy to conceive, how he should have considered the fourteenth, as critical of tertians, where the paroxysms happen on the odd days, and where the termination, as he acknowledges, constantly follows the solution of a paroxysm. The latitude likewise which he assumes, in explaining the apparent irregularities, is much too great. If we are permitted to reckon either the beginning or the termination of a paroxysm, as the critical period, according as it shall best suit our theory, it is easy to elude the most positive testimonies of experience. Yet, notwithstanding these defects, the different tracts of Galen on this subject, deserve to be carefully read. The facts they contain, though sometimes misapplied, are often important; and though we are not always satisfied with the reasonings of the author, we are astonished at the amazing mass of learning and knowledge found in his works.

There is little new information, on the subject of critical days, to be met with in the writings of those Greek physicians, who were posterior to the time of Galen. *Ætius Amidenus*, indeed, brings into narrower compass the substance of the doctrines of his predecessors. He mentions likewise, the most material of those circumstances, which influence the deviations from the regular crisis; but it is evident, that he has not sufficiently understood their application. *Alexander Frallianus*, who was an excellent practitioner, and a man of long experience, passes over this subject without particular notice; and though *Paulus of Ægina* has detailed the opinions of Galen in a more compressed form, than they are found in the original author; yet he has not added any new observations of his own. From the manner, indeed, in which he speaks, of the peculiar virtue of

the seventh and fourteenth, we should be apt to believe, that he is not altogether free from prepossession in favour of the Pythagorean numbers.

It was reasonable to have expected information, on the subject of critical days, from the writings of the Arabian physicians. The Arabians inhabit a country, where the periodical movements of nature are perhaps more clearly marked, than in our northern latitudes. Some districts of their country likewise were famous for the sciences at an early period, though it does not indeed appear that much of this knowledge descended in a direct channel to the Arabians of the present times. The Arabian physicians, in many instances, enriched medical practice with new forms of remedies; but they have for the most part only adopted the theoretical doctrines of the Greeks, particularly of Galen. Avicenna, the most famous among their physicians, and undoubtedly a great man, has Galen constantly in his eye: in short, he has done little more on the subject of critical days, at least, than merely translate the opinions and arguments of the celebrated Greek. He attempts, indeed, to be more explicit in ascertaining the date of invasion; but he does not in fact, go much beyond his predecessors;—hinting only obscurely, that the critical days ought to be calculated from the proper formation of the type, or the distinct invasion of the fever. He has added, however, that the odd days, are properly the critical days of the single tertian, and that the eleventh of course, obtains rank of the fourteenth in this disease.

There are many authors, who have written on this subject, since the arrival of science in Europe; but there are few that I have met with, who have thrown light on it from their own observations. The most of them have borrowed the opinion from Hippocrates; and accordingly have attempted to establish the truth of it, on the facts which are found in

the writings of that author; facts, which, on enquiry, will scarcely be found to be accurate enough to be made the basis of a general doctrine. It would be time ill spent, to enter into a detail of the arguments of this numerous list of writers; who, in reality, have oftener attempted to support their opinions by the authority of Galen and the ancients, than by the facts which might have been found in their own experience. From writers, however, of this description, it would be unjust, not to separate Hoffman, an author, who has related with candour the result of his own observations, in a practice of forty years and upwards. The facts which Hoffman mentions, throw considerable light on the subject; yet still they do not remove all the difficulty. They neither enable us to form an estimate of the power of the different critical days; neither do they at all assist us in comprehending the cause of the deviations. There are probably other modern authors besides Hoffman, who have treated of the power of critical days in fevers; but, except Dr. Cullen, I have not met with any one, who has left any observations which deserve much notice. This celebrated physician is a warm advocate of the ancient doctrine of critical days. He subscribes professedly to the arrangements of Hippocrates; though he adds likewise the result of his own observation, in the various kinds of fevers of this country.

The most eminent of the ancient and the most systematic of the modern physicians, all agree in ascribing to certain days a particular power in terminating; yet they do not so perfectly coincide in the arrangement they have given of those days, or in the causes they have assigned for the particular pre-eminence. The inconsistency of Hippocrates has, perhaps, been in some measure the source of this diversity of opinion. In one place, this author has ranked the twentieth as the proper critical day in fevers; in some others, this power is attributed to the twenty-

first. That the twenty-first is properly the day of crisis, was the opinion of Archegenes and Diocles; that it should be so, is not inconsistent with the general principle of the Hippocratic doctrine; viz. the movements of a quaternary period. So far is clear; but as it was observed by Hippocrates, as well as by other authors, that the twentieth was still more frequently a day of crisis than the twenty-first, a *προσθεσις*, on the fourteenth, was introduced to account for this apparent deviation from the general rule. This idea of *προσθεσις*, or accumulation of one period on another, which is mentioned in the writings of Hippocrates, originated perhaps in the doctrine of Pythagoras. It is adopted by Galen, and it appears in reality to be occasionally true; yet it can never be considered as an established principle in the movements of febrile diseases. By means of such accumulation, however, Galen has attempted to establish the pre-eminence of the twentieth, which he considers as the real critical day of Hippocrates. That the twentieth—(not the twenty-first) is actually the critical day of Hippocrates, is likewise decidedly the opinion of Dr. Cullen, who, going a step farther than his predecessors, endeavours to support his assertion by some arguments, which are entirely new. This ingenious author hazards the bold conjecture, that the appearance of the twenty-first, in the writings of Hippocrates, has arisen wholly from accidental error in the original manuscript: but with all due deference to such respectable authority, I must beg leave to suggest, that the twenty-first occurs too frequently in those writings, which have been ascribed to the Coan Sage, to give countenance to the opinion, that it owes its place, as a critical day, to careless error. The other argument is more ingenious; but perhaps not better founded. This writer has ventured to maintain, that the type of febrile diseases changes to quartan after the eleventh; but I can see

no good reason for the supposition. Medical writers have repeatedly noticed instances of crisis, on the thirteenth, and fifteenth; even my own experience, narrow as it has been, furnishes me with sufficient evidence, that crisis actually do happen at the above-mentioned periods.

As those days, which have been chiefly considered as critical, are now supposed to be sufficiently known, it will not be superfluous in the next place, to take a short view of the causes, on which the particular pre-eminence has been thought immediately to depend. The quaternary period, which in reality is a period of four, and a period of three days succeeding each other alternately, is the general principle assumed by ancient physicians, to explain this arrangement. But if we continue to pursue the undisturbed movements of a quaternary period, we shall bring the eighteenth and twenty-first into the order of critical days, rather than the seventeenth, and twentieth. The contrary is in fact the case. To obviate therefore this difficulty, or to reconcile observation with theory, a *προσθεσις* has been supposed to take place on the fourteenth. That a *προσθεσις*, or as it may be translated, the accumulation of the beginning of one period on the extremity of another, frequently takes place, cannot be denied; but its appearance is not determined by a fixed law. It is observed on the seventh, on the fourteenth; in short, on any day whatever. The quaternary period, with *προσθεσις* on the fourteenth, is the only principle employed by the ancients for explaining the usual arrangement of the critical days; yet I must observe, that it is capable of doing this, only in a very imperfect manner; it totally excludes some days of very considerable power. Dr. Cullen, sensible, perhaps, of this defect, suggested that there was a change from the tertian to the quartan type on the eleventh. This change, it must be confessed, explains with perfect plausibility

the pre-eminence of the fourteenth, seventeenth and twentieth; but there is the strongest reason to believe, that it does not in fact take place. I mentioned before, that instances are recorded by medical writers of crisis, which have happened on the thirteenth, fifteenth, and the other days, which are not included in the quartan period; and I can add from my own experience, that where the disease was of such a kind, that a type could be clearly traced; no such change, as this author has suggested, was ever seen.

Having ventured to declare, that the causes, which have been hitherto assigned for the pre-eminence of certain critical days in fevers, are extremely defective; the facts, which I have mentioned before, it is presumed, may enable us, if they are properly understood, to give a more satisfactory explanation of this singular phenomenon. There are few people of experience and observation, who do not know that the tertian is the most prevailing type in febrile diseases. This, at first sight, gives a general pre-eminence to the odd days; but though the tertian period prevails very generally in fevers, yet it must also be remembered, that these revolutions are sometimes completed in a shorter space of time than the regular period; whilst the types are frequently found to be doubled, or even more variously combined. In consequence of these accidents, apparent irregularities are often produced in the order of the critical days; though they may be all satisfactorily accounted for, by calculating the time by the periods of the disease, or by simplifying those types which are more evidently complicated. By attending to the circumstances I have mentioned, all the difficulties may be easily removed in periodical fevers; but as numerous instances of fevers occur, where no type can be clearly traced; so it is necessary in such cases to seek for some other principle, which may be capable of explaining apparent irregularities. There very seldom

perhaps happens an instance of fever of long continuance, where the symptoms do not undergo some change in the course of the disease. Those changes or revolutions are generally at considerable intervals, frequently at an interval of seven days. The circumstances by which those changes are indicated, are not by any means obscure; and, perhaps, there would not be great error, if we considered them as the commencement of a new complaint; at least by considering them as such, the general principle of the critical days is preserved consistent and uniform throughout. I shall mention such explanations as have occurred most frequently in my own practice. It often happened, that the symptoms of the disease underwent a material change on the fifth. It terminated on the ninth, or perhaps only put on a new appearance on the ninth, its final termination not happening till after another period of five days. In the same manner, a change of symptoms on the seventh, was followed by a crisis on the thirteenth; or if the change of symptoms was not observed till the ninth, the crisis probably did not make its appearance till the seventeenth. Such change of symptoms on the odd days, (where we may say with propriety enough, that one disease was accumulated upon another), there being seldom any previous marks of crisis, was by no means uncommon; yet it happened still oftener, that the paroxysm of the odd day declined; the original disease terminated imperfectly, whilst a new one began the day following, which was an even day. By such accidents the order of the days of crisis was changed: And from the last mentioned cause the fourteenth, as a second seventh, becomes remarkable among the critical periods of fevers. This idea of a second seventh occurred to me many years ago, and long before I was acquainted with the opinions of Hippocrates or of Galen. It now receives information from the testimony of these careful observers. There are many,

I make no doubt, who will be disposed to treat it with ridicule; but I shall combat their opinion with no other argument than a request, that they write down carefully the history of a tedious fever, and afterwards review its course without prejudice or partiality.

I now only beg leave to add, that the facts which I have mentioned in the preceding pages are circumstantial, and give room to conclude, that by simplifying complicated types, by calculating the time by the revolutions of the disease, or by beginning to date a second time from those great and remarkable changes, which happen at more distant periods, a doctrine is formed, perfectly uniform and consistent with itself. It is confirmed by every observation which I have been hitherto able to make. It is no more indeed, than an analysis of those several cases, which have occurred in my own practice; which in periodical fevers at least, has been tolerably extensive.

But though the prevalence of a tertian type, explains satisfactorily the general critical power of the odd days; and those other circumstances, which I have likewise taken notice of, account no less clearly for all the deviations, which are observed to take place; yet if we attempt to seek for a cause of this type, or of those changes, which happen at more distant, particularly at the septenary periods, our progress is soon stopt. Galen, who seldom hesitates in explaining the phenomena of nature, acknowledges here that he was unwillingly drawn to a discussion of the subject. The question undoubtedly is a difficult one; and, it is to be feared, must remain for ever unknown. In the East, where the powers of the human mind were not only earlier developed; but where men, from climate and modes of life, were led more early to observe the motions of nature, stated and periodical movements were soon discovered in the economy of the sublunary system. Egypt,

there is reason to believe, is one of the countries where these revolutions were first taken notice of; at least it was on the banks of the Nile, that the Greek philosophers first gathered the seeds of natural science. Among the knowledge or opinions, which these sages carried back to their native country, we may reckon the doctrine of the power of numbers; which though disfigured perhaps by the metaphysical genius of the philosopher of Samos, has observation in some degree for its basis. It does not concern us at present to enter into a particular discussion of this opinion; but as far as relates to the subject in question, we cannot refuse acknowledging, that the frame of man is liable to regular changes, at particular periods, comprehended in a certain number of days and hours. But though this general truth is indisputable, yet there is no argument which leads us to suppose, that those changes are, in any degree, influenced by an harmonic proportion in the simple number of the days. Ill founded however as this doctrine obviously is, it was in high fashion with the Greeks in the time of Hippocrates; and seems evidently to have had some influence on the opinions of this author. Without such a prepossession, indeed, it is not easy to conceive, how he could have fabricated the system which he has given to the world; as it by no means results from the facts which are found in his writings. Galen in this, as in most subjects, follows the footsteps of Hippocrates. He disclaims, I must confess, the *power of numbers*, simply as numbers having any effect upon the most usual days of crisis; but he maintains the influence of a quaternary period, which appears very plainly to be a remnant of the doctrine of Pythagoras. However, after exhausting himself, and fatiguing his readers with a detail of useless conjectures, he at last ventures to conclude, that the business of crisis is to be referred ultimately to the course and different aspects of the moon. The opi-

nion, like many others recorded by the Greek physicians, draw its origin from Egypt. It is not, perhaps, altogether without appearance of plausibility; yet I must add, that if the moon has in reality any influence in this business, the laws which regulate its effects are obscure;—indeed, not in the least understood. The conjecture however, fanciful as it appears to be, met with the general assent of medical writers, till about the middle of the sixteenth century, when Fracastorius, a man of ingenuity and elegant genius, attempted to substitute another in its place; though unfortunately, not a more probable one than that of his predecessors. This author, after a display of much learning and general knowledge, at last ventures to conclude, that the power of the different days of crisis, depends on peculiarities in the laws of motion of the different humours, which give rise to the different species of the disease: but with regard to this hypothesis, it is only necessary to remark, that while the very existence of the humours is doubted with reason, there can be no certainty in determining the laws of their motions. But though the opinion of Galen, and this of Fracastorius, are only vague and very questionable conjectures; yet they are the only ones, so far as I know, which have been offered to the public. The subject is too intricate, perhaps, ever to be explained. For though we clearly perceive that fevers are usually of a stated duration; yet we are unable to perceive, whether this duration depends on something inexplicable in the peculiar nature of the cause, which ceases to act, or changes its mode of action at a certain period; or to some imperceptible revolution in the human frame, which destroys in a given space of time, that particular aptitude between the state of the body and the morbid cause, in which the disease may be said to consist. This only we know with certainty, that where the febrile motions are violent and continual,

the disease hastens to a termination; where they are languid and feeble, or suffer long interruptions, its duration is often drawn out to an undetermined length of time. Thus continued fevers, with inflammatory diathesis and much viscular excitement, for the most part terminate decidedly in seven or nine days; while those with low and languid motions, with long and distinct intermissions, as the quartan, and even sometime the tertian, continue for months, and decline at last by slow and almost imperceptible degrees.

It may seem that I have treated very fully of the critical days of fevers; yet before leaving the subject altogether, there is one thing still which requires to be mentioned;—I mean the great proportion of fatal terminations, which happen on the even days. The even days were observed to be fatal in the proportion of three to one, in those fevers, which came under my care during the time that I lived in Jamaica. The fact, which is curious and hitherto I believe unnoticed, was discovered in the following manner. That I might the better trace the progress of nature through the whole course of the fever, a subject which then engrossed my chief attention, I visited often, and spent much of my time in the apartments of the sick. Among other things, I discovered the manner in which death more usually approached. The natural course of the paroxysm appeared generally to be finished, or the action of the febrile cause seemed actually to have ceased. The lightning before death, as it is termed, which has been generally attributed to the last efforts of dying nature was frequently seen to take place. This was even sometimes so remarkable, as to give flattering hopes of a favourable crisis; yet in a short space of time, the powers of life begun to fail, and at last were gradually extinguished, like an expiring taper. —The crisis, strictly speaking, happened on the odd days, equally the same in those who died, as in those

who recovered; only I had inaccurately, accustomed myself to refer the critical period to that moment, were the signs of crisis were first perceived; in the other, I had considered it as happening at the hour of actual death. Thus it was observed in those fevers which terminated fatally on the even days, that the powers of life, though irrecoverably exhausted, were not totally extinguished by the paroxysm of the odd day. This paroxysm, in short, seemed to decline after the usual duration. It left the body, in some measure, free from disease; but so completely deranged in the vital functions, that the action of living, though it often went on for a few hours, could not be continued long. In this manner, the hour of death was frequently protracted to the even day; yet death happened sometimes on the even days, from another cause. The decline of the paroxysm, which in many cases was hardly perceptible, in others was very plain. The disease terminated; but a new one recurring, after a short interval, speedily put a period to existence. In the mild fever of Jamaica, death usually approached in the gradual manner I have just described; yet in cases of much violence and malignity, the fatal termination was frequently on an odd day. In such cases the patient died in the height of the paroxysm, carried off by convulsions, apoplexy, or other accident.

Those authors, who, since the time of Asclepiades, have denied the power of critical days in fevers, are numerous; and many of them possess considerable authority in the medical world. Their opinions, however, cannot be considered as of great influence in the present case, though they may assert, that they never have observed the pre-eminence of any particular days in terminating febrile diseases; such an assertion means but little; unless its author convinces us, that he has adopted a method of investigation by which those regular movements, if they actually ex-

isted, could not fail to be discovered. Truth in the present case, can only be known from minute and careful observation; but a train of minute observation is not likely to be the work of a busy physician; and one, who is little employed, has not sufficient materials in his practice to engage his attention to a continued pursuit. I consider it as my own good fortune, to have been placed between the two extremes of idleness and too much business. In the country where I resided for some time, the movements of nature were generally so distinct, as to be observed without much difficulty; my practice likewise was sufficient to employ my mind, and not more than it could comprehend easily; so that I had sufficient leisure to write down, and to digest the observations which I have related above. They afford, if I mistake not, some facts which are precise and pointed; and which supersede a multitude of arguments. I will not venture to say, that they remove all the mystery from this dark subject; but I cannot help flattering myself, that they point out a road by which we may continue our investigations with success. The subject of critical days is of such importance, as to demand every attention. A knowledge of it gives credibility to our art; whilst ignorance in this respect is the source of perpetual mistake and disappointment. There are many physicians of the present day, who treat the idea of critical days with ridicule; but their assertions only afford an argument of their own precipitancy, and superficial observation. The man in reality, who pretends to cure a fever, without a knowledge of the critical periods of nature, is no less presumptuous, than the mariner, who undertakes to conduct a vessel through the ocean, without being instructed in the manner of calculating her course.

C H A P. IV.

OF THE GENERAL REMOTE CAUSES OF INTERMITTING AND REMITTING FEVERS.

THE general remote causes of intermitting and remitting fevers have been so fully investigated by several eminent writers, particularly by the industrious and learned Lancisi, that little remains to be added: nor perhaps should I have thought it necessary, even to have mentioned the subject, were it not to take notice of some opinions of the late Sir John Pringle, which appear to have been formed too precipitately; and which, I can affirm from experience, have been pernicious to the health of thousands. It would be a very needless ostentation to adduce the authority of the ancients, to prove the general source of the disease which is the subject of the present treatise. The historians, no less than the physicians of every age, do not entertain a doubt, that fevers of the intermitting and remitting kind, owe their origin to exhalations from swampy and moist grounds. Daily experience still proves it; and there are few men whose observations are so circumscribed, as not to know, that it is in the neighbourhood of swamps, and near the banks of fresh water rivers, that those disorders chiefly prevail. But though it is only in the above situations, that intermitting and remitting fevers are more peculiarly epidemic; yet it likewise deserves to be remarked, that, independent of the particular circumstances of soil and local situation, the endemic of champaign countries is subject, in a greater or less degree, to an appearance of periodical revolution. Mud and stagnant water, in every climate, possess the materials of the cause of this species of disease; but a combination of other cir-

cumstances is required to give them activity. Among the principal of those circumstances, which call forth this action, we may reckon the influence of a powerful sun. Hence, (as is commonly known), some situations, which, in the colder months of winter, are distinguished for no particular disease, in the hot months of summer and autumn, are observed to be most malignantly unhealthful.

The nature of this exhalation or cause of fever, though it has long been a subject of enquiry, remains still unknown. We plainly perceive it to be of various degrees of force, and in various states of concentration; and we can easily conceive it to be variously modified and combined;—but we go no farther. It has been said, to possess a septic principle; but this alone will scarcely be thought sufficient, to account for the very peculiar manner in which it affects the human race. Some other quality is necessarily joined with it, which our senses cannot lay hold of. But though the ingenuity of man has not hitherto been able to penetrate the intimate nature of this cause of fever, we still have it in our power, in some degree, to trace its effects on the human constitution. We plainly perceive that an habitual exposure to it, is peculiarly *unfriendly* to the principle of life, and in a very remarkable manner shortens the period of existence. In proof of this I mention from good authority, that white females, born and constantly residing in the lower districts of the province of Georgia in America, have seldom been observed to live beyond the age of forty. Males, sometimes approach near to fifty; while Europeans, who had arrived at manhood before they came to the country, often attain a good old age. The fact is curious, and shews, in a strong point of view, the deleterious quality of the air of those climates. But though the general nature of the country, which I have just now mentioned, is unhealthy in a high de-

gree; yet there are situations, in the Carolinas and Virginia, which are destructive of life in a still more remarkable manner. There is not on record, I am credibly informed, an instance of a person born at Petersburg in Virginia, and constantly residing in the same place, who has lived to the age of twenty-one. When the British army marched through this province, in the year 1781, I had the opportunity of seeing a native of this town, who was then in his twentieth year; but he was said to be the first, who had ever attained so advanced an age. He was decrepid, as if from the defects of time, and it did not appear that he could survive many months. Yet it is not a little curious, that this man had never been much confined with sickness. The residing constantly in the same pernicious air, seemed alone to have been sufficient so remarkably to accelerate decrepitude. But though the instances I have mentioned, afford sufficient proof, that this miasma is unfriendly to the principle of life; yet we are by no means instructed, as to the manner, by which it becomes so. This seems to be one of the arcana of nature; and it will profit little to prosecute it farther by conjecture. It will, however be an object of utility to mark the soils and situations in which the exhalation most abounds, and to trace the causes which heighten or lower its activity.

The history of the remote causes of intermitting and remitting fevers, with all the circumstances connected with them, having been, as I said before, so fully investigated by others, I shall only add a few cursory remarks, where the information does not seem to be sufficiently precise, or where the conclusions, which have been made, are not justifiable by experience. It is an opinion, which, though it did not originate with Sylvius de le Boe, evidently gained weight from his authority—that a mixture of salt with fresh water, as corrupting more easily, af-

fords a more noxious exhalation than fresh water alone. Lancisi has mentioned the observation; and Sir John Pringle considers it as an established fact; but the evidence, by which he attempts to support his opinion, is not decisive. It would be in vain to deny, that the neighbourhood of lakes or rivers, with a mixture of salt water, is often highly unhealthful; yet we may affirm with confidence, that it is seldom more so, than where the lakes and rivers are perfectly unmixed. In proof of this assertion, I might adduce the example of Savanna la Mar in Jamaica, or draw instances from the numerous islands on the coast of the Carolinas; where sea and river water are often blended together in various proportions; to which might be added, the more particular evidence of the relative healthiness of the banks of rivers. So far as I have observed, the usual endemic was less frequent, and less formidable on the banks of rivers, after their waters became mixed with those of the sea, than before this happened; unless the circumstances were in other respects more favourable for the production of the disease. Hence there is but little reason for supposing, that there actually exists any degree of mixture of salt with fresh water, at least of running water, which absolutely heightens the noxious quality of the exhalation. The above is an opinion of sufficient consequence to demand investigation: but there is another advanced by this celebrated author, worse founded, and of still greater concern, which I shall likewise mention. From an idea that a free circulation of air, is of all things the most essential to the preservation of health, Sir John Pringle enjoins in a very positive manner, not only that open ground, but that the banks of large rivers should be chosen, in preference to other situations, for the encampment of troops. This author's opportunities of information were good; his opinion has therefore gained weight, and his advice, I am afraid,

has been often fatally followed. It would be no difficult task to produce testimonies, from both ancient and modern history, of the unhealthiness of those situations, which Sir John Pringle has thought proper to recommend; but at present I shall confine myself to that, which has more immediately fallen under my own observation. The instance I shall mention, is only a single one; but it proves so clearly the danger of encamping on the banks of fresh water rivers, as to render all others superfluous. In June 1780, the first battalion of the 71st regiment was detached to the Cheraws, where it encamped on open ground, within five hundred paces of the river Pedee. The people of the country, taught by experience, suggested the propriety of drawing back the encampment into what is called the Pine-barren, assigning as the cause of their advice, that the distance, as well as the cover of the wood, might be a security against the damps of the river, which were observed to be extremely noxious in that climate. A position in wood, accessible on all sides, would not perhaps have been military; so that no alteration was made. The other battalion of the regiment joined in July. It arrived in perfect health, and encamped likewise on open ground; but still nearer the river. In a fortnight the intermitting fever began to make its appearance; and in less than three weeks, more than two thirds of the men were ill; whilst scarcely one of the officers had escaped. The officers, it must be remarked, encamped in the rear of the men, and immediately on the bank of the river, the course of which was uncommonly slow at this place; while its banks, though high, were oozy and foul. There are few instances on record perhaps, where a degree of sickness, greater than the present, has been observed in so short a space of time. The first battalion, however, did not suffer in the same proportion. The ground

of encampment was not only at a greater distance from the river; but being also nearer to a wood, many of those, who were not confined by their duty to a particular spot, found a convenient shelter in its shade, from the powerful heat of the sun. These I must not omit to mention, were the least sickly of the whole encampment. The above is an important fact. It proves clearly, that no ideal circulation of air can counterbalance the noxious exhalations from rivers; and it likewise affords a presumption, that instead of danger, there is safety in the shelter of wood. But with regard to this, no absolute rule can be given. It must generally be decided by local circumstances, whether wood, or open ground are to be preferred for the encampment of troops. Upon the whole, however, there are many reasons to induce us to believe, that as an encampment is not only more military in the body of a wood, than in open ground surrounded by woods; so it is likewise more safe with respect to health; particularly if within the reach of effluvia from swamps or rivers. The reason which offers is obvious. The wood not only stops the progress of noxious vapours carried from a distance; but it also covers the earth from the immediate action of the sun—the powerful cause of exhalation; in doing which, it perhaps, does more than counterbalance the less free circulation of air, or the greater dampness of the ground. But lest the authority I have mentioned, should not be thought sufficient, the opinion receives farther confirmation from the testimony of the ancients. Histories abound with examples of destructive epidemics, which have followed the cutting down of groves, which covered morasses, or which intercepted the progress of marsh exhalation. America also furnishes daily instances of a similar truth. In this country the unhealthiness of a place is often obviously increased, by cutting down the woods of the neighbouring swamps: hence no

rule is more liable to exceptions, than that which has been so generally enforced; viz. that clearing a country of its woods invariably renders it healthy: unless the grounds be drained and cultivated, as well as cleared, the effect is likely to be the reverse.

It would be curious and useful, could we trace this miasma or cause of fever in its progress. I do not deny that the noxious exhalation may be accidentally enveloped in fogs; but it is not necessarily so; and I add, that the dews of night, unless as an exciting cause, are less pernicious than has generally been imagined. Low grounds, in the same manner, are not always unhealthy; as high and dry situations sometimes afford no protection against the ravages of this disease. The situation of the encampment which the 71st regiment occupied at King's-bridge, in the year 1778, affords a curious and direct proof of the truth of this opinion. About two hundred paces to the right of the spot, on which the tents were pitched, was a tract of low and swampy ground; but the immediate situation was dry, and of considerable elevation. The right was *particularly* so; yet it was principally on the right, where the disease raged with violence. The left, though on low ground, over which fogs frequently hung till late in the day, suffered in a much smaller proportion. From this we might infer, that a dry and elevated situation is by no means exempted from intermitting and remitting fevers: but the great degree of sickness, which happened to those people, who not being confined by the nature of their duty to one particular spot, pitched their tents on a hill in the rear of the encampment, proves it clearly. The ground, which those persons made choice of was directly in the tract of air, which blew over the swamp. It was dry and scarcely ever covered with fogs; yet there was not an individual among them who encamped upon it, who did not suffer from this raging epidemic. The present in-

stance, with many others which I might adduce, leaves little room to doubt, that instead of exposing encampments to streams of air, which blow from rivers or swamps, it ought to be our principal business to guard against those noxious effluvia, by the interposition of woods or rising grounds. Exhalations which are the causes of fevers are subtle, and seem to be pernicious, chiefly in their ascent:—visible damps or night dews are comparatively innocent.

So great is the importance of preserving the health of an army in the field, that the choice of encampments ought to be made a subject of particular enquiry. The opinion of Sir John Pringle on this head, (which, in fact, is an opinion of theory rather than observation), has been followed too long without examination. The directions of this author are influenced wholly by the dread he entertained of a contagious or hospital fever; but a contagious fever, is seldom a disease of the field; and has, perhaps, scarcely ever been known to make its appearance in a moving camp. Diseases of the field are often epidemic, sometimes malignant, but rarely contagious. I even doubt if the dysentery, whilst a camp-disease, is so in any remarkable degree. It was not so at least in America, in those campaigns, where I had the opportunity of knowing the state of the army.

The general remote cause of intermitting and remitting fevers, consists, as was mentioned before, in invisible exhalations floating in the air. These are more copious in some situations than in others; and appear to be rendered more or less active by a great variety of causes. Among the number of those causes which have been accused of exciting fever, it has been usual to reckon excess in drinking. It cannot be denied, that this cause, in several cases, has brought forth the disease, when it probably would not have otherwise appeared; yet it has been likewise observed that a debauch of wine has sometimes restored the

body to health, when languishing under the influence of this disorder in an obscure or irregular form. The moderate use of wine, however, has been generally recommended as a preservative in times of great heat, and epidemic sickness:—and under limitations it undoubtedly is of use. In a time of very pressing calamity, the oracle of Delphi gave its sanction to the prescription, and history bears testimony to its success. But besides excess in drinking, cold and fatigue have likewise been considered among exciting causes of fever. In short, whatever exhausts or diminishes the activity of the powers of life, may be justly viewed in this light. Yet still I must observe that neither cold, fatigue, nor any of the causes of this train, give occasion to a proper intermitting or remitting fever, unless the predisposition to the disease be particularly strong. As a proof of this, I must beg leave to mention a fact, which fell under my own observation. In an expedition into South Carolina, in the year 1779, a part of the army was near five hours in passing Purisburg swamp. The men were always up to the middle, sometimes up to the neck in water. The cold and fatigue were both very great, and a fit of intermitting fever was the consequence in a great number of the soldiers: yet it was only in a few instances that the disease went through a regular course, though there was even a general pre-disposition to it, in the habits of almost all the men who composed the detachment. The most of them had suffered from it severely the preceding autumn; and a temporary return of it, was generally observed to follow any extraordinary exertion, or the application of a debilitating cause. The above causes are generally reckoned exciting causes of fever; but besides these there are several others of considerable power, which as being commonly known, I shall not now spend time in enumerating. There however still remains one, which, though

very universal, and perhaps more powerful than any other, has hitherto been little attended to. The approach to the new and full moon, in some degree, perhaps in every part of the globe, but particularly in the West-Indies, appears to be connected with the invasion and relapse of fevers, in a very remarkable manner. This observation has been hinted obscurely by one or two authors; the idea has been treated with ridicule by others: and it must be confessed, that the facts, which have hitherto been produced in support of the opinion, are extremely vague and equivocal. I shall therefore enter a little more minutely into the subject, and state circumstantially the evidence, from which I have been led to consider the approach to new and full moon, as a powerful exciting cause of fever.

That the moon exerts some influence on the human frame, and that her different appearances are more or less connected with the progress and issue of diseases, does not seem to have altogether escaped the notice of the ancients. In a fragment of Hippocrates, in the edition of Vander Linden, we find a detail of the different aspects of the moon and planets, with their combined influence on the fate of diseases; but the style and manner of this little tract are so perplexed, that I do not pretend to understand its meaning. Galen had likewise some obscure ideas on the subject; but he has left us nothing clear and explicit. The Arabian writers are also confused and inaccurate; so that the first circumstantial evidence of the influence, or connexion of the moon with the human body, is found in the works of Ballonius, a French physician of the sixteenth century. The fact which this author records, though not altogether in point, is curious. A Parisian lady of quality appears by the account of Ballonius, to have been very singularly affected during an eclipse of the sun. Her complaint threatened nothing dangerous, and her physicians

were amusing themselves with observing the progress of the eclipse, when they were suddenly summoned to her assistance. In the moment when the eclipse was deepest, she had the appearance of dying; but these threatening symptoms decreased with the decrease of the eclipse; so that she at last returned to her former state. This is only a solitary instance, and perhaps might be reckoned accidental. We may however add to it the general testimony of Ramazzini, who lived at Modena in the beginning of the present century. This author's observations, indeed, are by no means precise; yet he was convinced by them, that the course of Epidemics was considerably influenced by the particular state of the moon. It is almost needless to mention Dr. Mead, who wrote a treatise expressly on the moon's power on the human body. The facts which this writer has collected, afford a reasonable presumption, that this planet is not without some influence in several diseases to which man is liable; but we find not any thing in the work, which particularly relates to fevers. I shall mention a fact recorded by Dr. Grainger. It is the most circumstantial I have yet met with; and the strongest to be found perhaps in the writings of any European physician. Dr. Grainger, who was a surgeon of the army, served in the Netherlands about the years 1746 and 47, and wrote a treatise on the intermitting fevers of that country. Among other observations he takes notice of a circumstance which occurred to him at that time, and which he then considered as singularly curious; viz. that twenty of the men of the regiment, of which he had the charge, were seized with this fever, which was then epidemic, on the day of a solar eclipse. He has not made any application of the fact. It furnishes however a very substantial evidence, of the influence or connexion of this planet with the invasion of febrile diseases.

It appears to have been long known in India, that

fevers have a tendency to relapse about the new and full moon, and particularly at the time of eclipses but Dr. Lind of Windsor is the first, who brought the knowledge of the fact to Europe. In an inaugural dissertation, published at Edinburgh (I do not exactly recollect the year), this author observes, that this opinion prevailed very generally in the East. He adds likewise, that some instances occurred in his own practice, which gave him cause to believe that the fact was well founded. Dr. Lind continued of this way of thinking for several years after his return to England. He does not indeed at present deny the fact. He only suggests that it may admit of a different explanation, from that which he had given in his first publication. The spring tides, as they overflow the low grounds, according to his present opinion, afford a more probable cause of the uncommon increase of fevers about the new and full moon, than the direct influence of the planet itself. I will take the liberty however to add, that this opinion has been offered to the public, from a very imperfect view of the subject. I can affirm, even from the confined circle of my own experience, that a connexion, between the moon and the invasion of fevers, certainly takes place in districts remote from the sea; and I believe it is generally known, that a fever, or the paroxysm of fever, is not commonly the instantaneous consequence of exposure to its remote cause; which ought to be the case, if this author's reasoning were just.

The next, and indeed the only author who has written professedly on the influence of the moon in fevers, is Dr. Balfour; a gentleman who resided several years in India, and who practised with reputation in the service of the Company. This author pretends to have investigated the subject with care and attention; but there appears in reality, to be more theory and general assertion in the treatise than circumstantial

fact. The result of his observations he informs us, amounts to this: viz. that the three days which precede, and the three days which follow new and full moon, are remarkable for the invasion and relapse of fevers; that the day of the full moon, and the day of the change of the moon, are the most remarkable of all: and farther, that the days which follow, are, in general, more remarkable than those which precede.

I have now brought together the substance of what is found in the writings of those authors who have mentioned cursorily, or treated professedly of this subject. There is not in any part of it, if we except the instance recorded by Dr. Grainger, any thing accurate and precise enough to enable us to form an opinion. What has fallen under my own observation, I would flatter myself, is less ambiguous; and though it may not be so explicit, perhaps, as to establish the doctrine completely, it may at least assist us, I hope, in approaching nearer to the truth.— I shall relate it in a few words.

When I arrived in Jamaica, in the year 1774, I had no other knowledge of the influence of the moon in fevers, than what I retained from a cursory reading of Dr. Lind's dissertation. I remember, however, to have mentioned the circumstance to several practitioners, who had lived many years in the island. As I conceived there was a similarity between the climates of Jamiaca and Bengal, I thought it not improbable, that some of the practitioners of the country in which I then was, might supply me with satisfactory information on the subject. There were none of them, however, who acknowledged that they had ever observed any connection between the moon and febrile diseases; neither were there many of them, who seemed disposed to give credit to its existence. Twelve months or more elapsed without my having paid any further regard to the fact, when an acciden-

tal relapse of fever, happening near the time of full moon, recalled Dr. Lind's observation to my memory. It likewise brought to mind a circumstance, which till then I had overlooked. I had seen frequently, though without attending to it particularly, that three or four of the soldiers of a company of the 60th regiment, who were quartered at Savanna la Mar, and of whom I had the care, were attacked with fever on the same day; whilst it seldom happened, that any other febrile illness made its appearance in the garrison, for the ensuing fortnight. This having been observed oftener than once, at the time the moon was near full, a hint suggested itself, that the cause, which was said to influence relapses in India, might here have an effect on the original invasion. But in order to ascertain the truth of this conjecture, which I considered as a matter of some importance, I provided myself with the almanack of the year 1776, and marked, in the blank leaf of it, the precise date of attack, of all those fevers which came under my care. In looking over those memoranda at the end of the year, I found I had put down thirty cases of proper remitting fever, the invasion of twenty-eight of which was on one or other of the seven days, immediately preceding new or full moon; that is in the second and last quarters. The same plan of observation was continued through the following year, and the result, though not exactly the same, was similar. Of twenty-eight cases, which were found in the almanack, twenty-two were in the periods above-mentioned: that is in the second and last quarters of the moon. It deserves however to be remarked, that three of those six cases, which were not in the common period of invasion, happened actually on the day of new moon;—a few hours after the change had taken place. But besides those cases of proper remitting fever which I have mentioned,

there were likewise found in the almanack many days fevers and slight feverish disorders, the invasion of the greatest number of which was likewise in the usual period.

The above is a literal state of the case as it stood in the almanack:—some remarks and observations, however, were added, of which the following are the principal: viz. That, though the whole of the second and last quarters of the moon is included in this period of invasion; yet the four days immediately preceding new and full moon, were more particularly distinguished for those febrile attacks: that in the dry season, which is reckoned the most healthy, the time of invasion was more closely connected with the new and full moon, than in the wet and sickly months, particularly when the sickness was epidemic, or of a bad kind: and lastly, that this influence, or connexion was more apparent in the soldiers of the garrison, who were exposed to few occasions of disease, excess in drinking excepted, than in the inhabitants of the town and country, whose occupations carried them oftner to places of unhealthy situation; or whose modes of life obliged them to submit to more various hardships or to greater fatigues than fell to the lot of a soldier in times of peace.

I shall further beg leave to add, that I went to join the army in America, in the year 1778; and that I continued in that country, the train of observation on this subject, which I had begun in the West Indies. The regiment, in which I served, was encamped during the months of June and July on a healthy part of York-island. Fevers were rare; and the time of invasion, of such as did appear, was chiefly confined to the second and last quarters of the moon. In the beginning of August, the encampment was removed to King's-bridge, where it occupied a very unhealthy situation. The intermitting fever soon

made its appearance. It extended in some degree to the whole battalion; but raged with particular violence on the right, which bordered on low and swampy ground. The approach to new and full moon never failed, even in this climate, to increase the number of the sick; yet it deserves to be remarked, that this increase was always smaller in proportion, in that part of the battalion, which lay contiguous to the swamp, where the disease was highly epidemic, than in the other extremity of the encampment, where it prevailed in a less degree. But still upon the whole, when the regiment moved from their ground, in the beginning of November, of a hundred cases of intermitting fever, which were marked in the almanack, eighty were found to have commenced in the usual period of invasion; that is, in the second and last quarters of the moon. It is somewhat remarkable, that relapses were in a smaller proportion. This regiment, some parts of the medical history of which I describe, embarked on an expedition for the southward in November, and arrived at its destination in Georgia, in the latter end of the year. It remained in the southern provinces, and served every campaign till the capitulation at York-town. The same train of observation was continued during this intervening space, and the same influence of the moon seemed in general to prevail; but the notes having been lost, I cannot now exactly ascertain the degree in which this influence took place. Of this, however, I am certain, that even in times of the greatest epidemic sickness, when the connexion was evidently weakest, the number of the sick was generally doubled in the periods approaching to new or full moon.

We cannot avoid concluding, from the facts which I have stated above, that the approach to new and full moon, or something connected with that approach, may be justly considered as a powerful exciting cause

of fever. The circumstances, indeed, which I have mentioned, are so clear and unequivocal as to leave little room for doubt: nor did I entertain any, till I found that the observations of Dr. Balfour, on this subject, were so strikingly different from mine. Bengal and Jamaica are distant from each other; yet few people will be disposed to believe, that so great modification of a general cause has arisen solely from this diversity of climate. Dr. Balfour must speak for himself. For my own part, I can only say, that what I saw I have related with truth. As I have told the manner in which the idea arose, with the manner in which the investigation was conducted, I leave the conclusion to be formed by the reader.

C H A P. V.

THE PROXIMATE CAUSE OF FEVER.

THEORIES of the proximate cause of fevers, or more properly modifications of theories, are so numerous, that a whole volume would scarcely be sufficient to give any tolerable account of them. It is a task indeed which I shall not undertake; yet I hope it will not be altogether superfluous, to give a cursory view of the principles, which have directed the conjectures on this subject in different ages. The principles are, in fact, fewer in number than at first sight they appear to be. Physicians, ambitious of raising their name and reputation, have shown great industry in multiplying and modifying opinions; yet it does not appear, that they have produced any great variety of theories, which are fundamentally distinct.

The ancients, who were little acquainted with chemical principles, or with the qualities and properties of the nervous system, placed the proximate cause of fevers in some signal symptom of the disease, such as increased heat, or abounding bile; or entering still farther into the fields of speculation, ventured to attribute it to derangements in the permeable canals of the body, or to affections of the humours, or circulating mass of fluids. Hence obstruction of pores, plethora, error loci, lentor and visciditas, or putrefaction of the humours, have all severally, at different times, or by different authors, been considered as the immediate or proximate causes of this disease. The theories, which prevailed in the schools till the beginning of the sixteenth century, did not often extend farther than to the causes which I have mentioned: but after that period, the discoveries of

the famous Paracelsus opened a road to innovation in medical reasoning. The followers of this author, if not numerous, were enthusiastic and vociferous. They indulged in the wildest extravagance of conjecture; and their opinions, for a considerable time, were combated with the authority of Galen, rather than with solid argument and accurate reasoning. At last the disputes between Chemists and Galenists beginning to subside, the chemical theories became incorporated with the doctrines of the mechanic philosophy, which were revived more than a century ago, and which still maintain some influence in the common systems of physic. In the mean time happened the important discovery of the circulation of the blood; but no immediate change, in the manner of accounting for fevers, ensued immediately in consequence of it. Yet as from this period the researches of physicians began to be conducted on a more extended plan, some parts of the system were brought into view, which had been formerly little attended to. The nervous system, which had been in a manner overlooked for many ages, was now found to be of importance in the economy of the animal machine; and authors soon began to consider it, as affording a probable seat for the proximate cause of fevers. Among the first of those authors, who viewed it in this light, we reckon Borelli and Dr. Cole; the one of whom in Italy, the other in England, proposed much about the same time, new and different opinions about the proximate cause of fevers. Their conjectures, I must confess, are far from being probable;—(that of the Italian is scarcely ingenious) yet they deserve to be mentioned in this place, as being among the first attempts to bring into view a part of the system, which is very essential in enabling us to account for many appearances in febrile diseases. It is commonly believed, that the nervous system was not discovered to be a part of material import-

ance; either in the functions of health, or in the affections of sickness, till the last century. This, in fact was generally the case; yet I must not omit to mention, that we meet with an expression in the writings of Hippocrates, viz. *τα ορμωντα η ανορμωντα σωματα*, which might incline us to be of opinion that this physician was not altogether ignorant of the influence of the nervous power; and that he actually considered this principle of the constitution to be of much importance in the management and cure of diseases. After Hippocrates, Van Helmont, under the whimsical appellation of Archeus, asserted more directly the dominion of the sentient principle. He has indeed applied its operations more particularly to assist him in explaining the theory of fevers; but it has been a misfortune that the opinions of this author have been generally less attended to, than perhaps they deserve: so that it has been customary to consider, the celebrated Hoffman as the first, who suggested the idea, that the proximate cause of fever depends on a derangement or affection of the nervous system; at least he is the first, who delivered a system on the subject, which can in any degree be considered as rational and consistent.

It will not be an easy task, to give a clear and distinct view of that, which has been considered by the ancients, as the proximate or immediate cause of fevers. The language of the earliest writers is not by any means precise in this respect; and we shall frequently, perhaps, have difficulty from the ambiguity of expression, to distinguish from each other the definition, the remote and occasional, or the immediate and proximate cause of the disease. The proximate cause of a disease, it must be remembered, is a cause which constantly and uniformly produces its respective complaint; and without which this complaint cannot even for a moment exist. It is, in short, the first essential derangement, which the ac-

tion of this cause produces in the frame of the sufferer: but though we know this to be certainly true, yet we have made no progress in discovering the nature of this derangement. The first action of the cause of fever is obscure, and some part of the derangement which it occasions, has hitherto probably passed over unnoticed, even by the most accurate observers.

If we attempt to give a view of the successive conjectures, which, at different times, have been offered to the public concerning the proximate cause of fevers, it will be necessary to begin with Hippocrates. We may collect very clearly from the writings of this author, that an increase of the heat of the body had afforded, to the still more ancient physicians, the first idea of the essence or immediate cause of fevers. This seems to have been the idea of the most ancient professors of medicine. Hippocrates in some degree subscribed to it; yet this author seems likewise to doubt, if the simple increase of heat alone is sufficient to constitute a proper fever, or that it can with propriety be considered as the essential proximate cause of the disease. But though Hippocrates raises this objection to the common opinion concerning heat, yet he still leaves us in doubt with regard to the opinion which we ought to adopt. His ideas are fluctuating and uncertain. We find in the different parts of his works, obstruction, plethora, miasmata or bile, all separately considered, as immediate causes of fever. But such causes, I may add, where they do take place, are in fact only more remote or distant causes. Neither miasmata, bile, nor obstruction, are circumstances on which the existence of fever invariably and necessarily depends; at least such causes require to be in a certain state of modification, which is yet undefined, before they are capable of actually producing the disease. Bile bears a very conspicuous part in the Hippocratic

doctrine of fevers. The fabric, indeed, which our author raises on this principle, is fanciful, and in many respects, ill founded; yet, as modified by the fertile genius of Galen, it passed on through a succession of many ages: nor is it, even now, altogether banished from the language of practitioners.

Such are the hints concerning the causes of fevers, which I have been able to collect from the writings of Hippocrates. The expressions are often obscure or equivocal; and we can scarcely say, that an opinion can be formed from them which deserves the name of a theory. The successors of this great physician were, perhaps, too sensible of this defect; and therefore attempted to fabricate other opinions, which might be more explicit and distinct. Among the first of those attempts, we may reckon the hypothesis of Diocles of Carystus, a physician who lived at an early period, and who was highly esteemed by his contemporaries. Fever, according to this author, is not so much a primary disease, as a symptom of some other affection. Wounds, tumours, and many other accidental causes, have certainly been observed to give rise to symptoms which have been usually denominated fever; yet neither wounds nor inflammations have been generally observed to give rise to a proper fever. I will not however deny, that wounds, or inflammations, occasionally prove exciting causes of proper fever, where there is a strong disposition to the disease, existing in the constitution, at the time those accidents have happened. It does not appear that this theory of Diocles gained much ground with succeeding writers; yet it was, perhaps, the cause of introducing the distinction of primary and symptomatic into the history of fevers; a distinction, which is frequently of consequence in practice. But I must further add, that though the opinion of Diocles is not admissible in its literal meaning; yet, in a modified sense, it is not altogether without foundation.

The symptoms of fevers are undoubtedly indications of a derangement of the body from its healthy state; but when we have said this, we can say no more.—The nature of the derangement, which in its first beginnings is not obvious to the senses, neither the ancients, nor the writers of the present age have, as yet, been able to ascertain.

Not very long after Diocles, Erasistratus, a native of the island of Cea, and physician at the court of Antigonus, furnished a conjecture concerning the cause of fevers, which is mentioned both by Celsus and Galen, and which appears to have originated in his anatomical researches. As Erasistratus directed his pursuits particularly to the sanguiferous system: so impressed, perhaps, with an idea of the importance of that part of the body on which his thoughts had been chiefly employed, he ventures to hazard the opinion, That the immediate cause of fever depends on a certain error loci, or transfusion of the red blood into the arterial channels: and this, he moreover adds, proceeds from repletion.—The opinion originates from an anatomical error, and on that account need not detain us any longer.

The next author, of whose opinion on this subject any distinct traces have been transmitted to us, is Asclepiades, the Bythinian, a man who seldom treats the doctrines of his predecessors with respect. In his rage for innovation, Asclepiades attempted to change or modify the theories of those who had gone before him, in such manner, as to hope to impose a conjecture on the world, which might, at least possess some exterior claims of novelty. He allows with the most ancient physicians, that the inseparable sign of fever, or its essential part, consists in an excess of heat; but having adopted the doctrine of atoms, which was conveyed to the Greeks by Democritus of Abdera, he pretends to account for the difference of types by a difference in the size of the corpuscles, which he

supposes to be formed by a combination of indivisible atoms. Thus we see that obstruction in the permeable canals of the body, in this writer's opinion, constitutes the theory of the proximate cause of fever: on which principle we may likewise conclude, that the modern doctrine of lentor and viscosity has built its foundation.

The author, whom I have last mentioned, may actually be considered as the original founder of the methodic sect. The principal tenets of this sect of physicians have been transmitted to us by Celsus, Cælius Aurelianus, or Galen; but the doctrines, which they promulgated, have not been very fully and perfectly explained. The great division of Themison, into *strictum et laxum*, furnishes a very simple view of diseases. Fevers are included in the first order of derangement; and in this respect, may be considered as depending on a cause similar to the obstruction obscurely hinted by Hippocrates, or more explicitly described by Asclepiades. There is this difference, however, between these respective opinions, that the earliest writers seem to have referred the obstruction to some change in the humours or circulating mass; while the *methodics* appear to have attributed it more directly, to a change in the capacity of the containing vessels. Hence we may infer, without any improper latitude of interpretation, that the *strictum* of Themison and Thessalus comprehends the spasmodic constriction of capillaries, which has lately made so conspicuous a part in the theory of febrile diseases. This theory of the methodics, where the nervous and fibrous system have been more regarded than the humours, or circulating mass of fluids, was principally followed at Rome, for more than a hundred years. At last Galen, who was a very unqualified admirer of Hippocrates, exerted himself so successfully in reviving the humoral doctrine of his master, that the methodic sect began to sink rapidly into

decay; and after a short time its traces were totally obliterated.

The frequent blanks, in medical history, make it no easy task to give a connected view of the fluctuating systems of the ancient physicians. The works of every writer of the methodic sect have perished, except those of Cælius Aurelianus: neither have we been able to discover any new opinion, or modification of opinion, concerning the proximate cause of fevers, between the time of Asclepiades or Themison, and the great commentator of Hippocrates, except that of Athenæus. Athenæus, who was the head of the sect of Pneumatics, stood high in esteem among his contemporaries and successors. This author ventured to suggest a new hypothesis, or more properly perhaps, only extended, and more fully explained a doctrine, of which the obscure traces may be discovered at an earlier date. The general cause of fever, in this writer's opinion, consists in a putrefaction, or putrescent state of the humours. Hippocrates seems to have entertained some indistinct idea of the same kind; and those, who have been inclined to this way of thinking, both in ancient and in modern times, have neither been few in numbers, nor contemptible in authority.

Galen, who has written on most parts of medical science more learnedly than his predecessors, has discussed very fully the subject of the proximate cause of fevers. Amidst the luxuriance of this author's colouring, it is sometimes difficult to lay hold of the precise idea; at the same time, that it is oftener tedious than instructing to follow him through the maze of his fanciful and inconclusive reasonings. I shall not therefore enter into a minute detail of his arguments; but still I conceive it may be useful, particularly to those who have not the opportunity of consulting his voluminous, and in some respects ill digested works, if I compress into narrow compass the lead-

ing principles of his general doctrines. In the first place, the opinion, hinted by Hippocrates and adopted by most of his successors, that the essence of fever consists in a certain derangement of heat, is expressly maintained by Galen, who explains more elaborately than his predecessors the various circumstances, which influence or modify this general cause of the disease. Galen assumes, indeed, as a fundamental position, that heat any how, or any where excited, communicated to the heart, and from the heart to the rest of the body, constitutes a fever; yet he afterwards adds more explicitly, that a preternatural heat does not constitute a fever, unless it is communicated to the heart; which is consequently to be considered as the principal seat and residence of the febrile affection. Having, as he imagines, established this fundamental principle, he proceeds to investigate, more particularly, the parts of the body where the heat resides, and the causes by which it is generated, propagated, or so modified, as to produce the disease in its different forms. But, that he may the better explain his meaning clearly, he divides fevers into three different kinds: viz. the hectic, or habitual, the humoural, and the ephemeral. The first he supposes to arise from an affection of the solids, or containing parts; the second from some derangement of the fluids, or contained parts; and the third from some disturbance of the spirits, or that part of the frame which we, perhaps, now distinguish by the name of nervous system. He adds in the next place, that putrefaction is the medium, by which fever is excited, where the fluids or humours are the subject of the disease, contiguity and continuity, where the illness affects the habit or solid parts; and where the effects are transitory and fleeting, he attributes the cause principally to the rapid movements of the spirits, or nervous influence. And lastly, he attempts to complete his theory, by explaining the different types of humoural

fevers, on the supposition of a state of putrefaction in the different humours, from which he supposes the disease to arise. On this subject he has deviated very materially from his master Hippocrates, though he probably drew his ideas from the hints, which are found in that author's works. Hippocrates explains, or attempts to explain the various types of fevers, by a simple difference in the quantity of the bile. Galen, on the contrary, as we have said just now, endeavours to account for this phenomenon, by a supposition of putrefaction in the phlegmatic and bilious humours, which bear so conspicuous a part in his theoretical system. Thus Galen supposes, that a putrescent tendency in the blood gives rise to a continued fever; a similar disposition in the phlegm disposes the disease to appear in a quotidian form: putrefaction of the yellow bile determines the type to be of the tertian kind; whilst a like tendency, in the black bile, regulates the movements of the quartan period.—It is unnecessary to make any remarks on the baseless fabric, which this author has offered to the world, concerning the proximate cause of fevers.—Its inconsistency and insufficiency are perfectly obvious.

After the time of Galen there does not appear to have been any material change, in the manner of accounting for fevers, for many ages. Aetius Amidenus indeed suggested some restrictions and explanations in certain species of fever, which do not seem to have been so explicitly marked by the commentator of Hippocrates. Instead of considering putrefaction as the sole means of exciting heat in every species of humoral fever, Aetius ventures to insinuate, that there is no state of actual putrefaction in the *συννοχος*, or that species of disease which is purely inflammatory, the cause of which appears to be simply an inordinate fermentation or ebullition of the blood. But except in this instance, the succeeding Greek physicians do not seem to have departed, in the least, from

the direct footsteps of Galen. The Arabians likewise, among the principal of whom we may reckon Avicenna, adopted his general doctrines, and modes of reasoning, only Avicenna defines more expressly than others had done before him, that fevers of all denominations arise immediately from a preternatural heat of the heart; in doing which, he seems to have extended the influence and power of that quality which preceding authors in looser terms had considered as the general cause of febrile diseases.

The doctrines of Galen, with some immaterial innovations of the Arabian physicians, wholly occupied the schools of medicine, till the beginning of the sixteenth century, about which time Aureolus Philippus Theophrastus, commonly known by the name of Paracelsus, effected a revolution of opinions, which marks an important period in the history of the medical art. Paracelsus, who was a man of a singular turn of mind, spent the earlier part of his life in travelling among the nations of Asia; where he probably acquired some knowledge of chemistry, in which science the Arabians appear, even at that time, to have made considerable progress. The knowledge, which Paracelsus carried home to his native country, was not generally known in Europe. This author applied it with success in the cure of some desperate diseases; and acquired uncommon fame from his new and unheard-of remedies. He was an empiric in the theory, no less than in the practice of the art; and I may add, that his attempts to overturn the doctrines of the ancients, give an indication of more effrontery than genius or knowledge. The wonderful cures of obstinate diseases, which he was said to perform; and still more, perhaps, the mysteriousness of his language, which caught the notice of the vulgar, who often imagine that knowledge is concealed under terms, which they do not understand, brought followers to his standard. Thec-

ries of the proximate cause of fevers, were fabricated without difficulty, by the help of those principles, which Paracelsus had introduced to the acquaintance of the world; yet it does not appear, that any theory arose, during this period, which had probability, or even ingenuity for its support. The period indeed, during which chemical reasonings so universally prevailed, may be styled justly enough a period of medical romance: and I should consider it as a trespass on the patience and good sense of the reader, to spend time in refuting the absurd and incongruous doctrines of sulphur, nitre or mercury; acid and alkali, or the various modes of fermentations, which for a time filled the writings of physicians. The mechanical mode of reasoning, which succeeded, or rather which became incorporated with the doctrines of the chemists, seemed at first to promise greater advantages: but though theories of fevers were formed by many eminent men, both of the last and present century, on the principles of the mechanical or chemico-mechanical philosophy; yet there are not any of them, which seem to have afforded a satisfactory explanation of the subject.—The so-much celebrated doctrine of lentor and viscosity was assumed without evidence of its existence, and persisted in, without being sufficient to account for the phenomena of the disease.

Those conjectures concerning the proximate cause of fevers, which I have mentioned hitherto, can seldom be said to extend farther than to a particular state of the humours, or circulating mass of fluids, which, according to the prevailing philosophy of different ages, have been supposed to be changed from their natural and healthy state, by chemical or mechanical derangement. I observed before, that it might appear, from an accidental expression in the writings of Hippocrates, that this author was not altogether ignorant of the influence or effects of a nervous power, or sentient principle. The methodic

fect likewise, may seem to have comprehended in the idea which they have given of diseases, that there is some derangement of the fibrous system; or perhaps that a spasmodic stricture of capillaries is actually the immediate cause of fever; whilst Galen every where celebrates the powers of nature or vires naturæ medicatrices, which bear in his opinion, a very active part in the cure of febrile diseases. To those vague ideas of the ancients, we may add the more modern and explicit doctrine of Van Helmont, who was a man of genius, learning and observation. Van Helmont adopted the sentient principle of Hippocrates; but he also applied it in a bolder light than had been done by its original author, and employed its assertions more particularly towards the explanation of the cause and phenomena of fevers. The enthusiasm of this writer disgusts the philosophic spirit of the present age, and we must acknowledge, that his ideas are often unphilosophical and absurd; yet we must likewise do him the justice to add, that the principle of his doctrine in some degree is well founded, and that his views, in many respects, are important in practice. I must further observe, that the efforts of nature, so celebrated by Campanella and Sydenham, and even, perhaps, the *αὐτοκρατεία*, of Stahl and his followers, can only be considered as modifications of the furious Archeus.—But though the authors I have mentioned, seem evidently to have possessed some vague idea of the powers or influence of the nervous system; yet there are not any of them, who have attempted to explain its operations by a philosophical and consistent mode of reasoning. The period of this improvement is not very remote.

As soon as the circulation of the blood was known and fully established, the heart lost some part of its former importance; whilst the brain and nerves, which for many ages had been little regarded, rose into primary and essential consequence. But

though the brain and nerves were discovered, soon after this period, to be the instruments of life and motion; yet the laws of this part of the system were at first only imperfectly understood; and the attempts to explain its operations were, for a while, whimsical and absurd. Willis deserves some credit, as being one of the first who brought the general importance of the nervous system into view: but Borelli, an Italian mathematician, actually appears to be the first who ventured to ascribe the proximate cause of fever, to a particular derangement of this part of the frame. The immediate cause of fever, in this author's opinion, depends on some unusual acrimony of the nervous fluid; but it is only necessary to observe with regard to this doctrine, that a supposition of acrimonious fluids, where a fluid cannot be proved to exist, is so perfectly visionary, as only to deserve to be mentioned, from its being the first attempt to bring this part into view, in accounting for febrile disease. This hypothesis, however, though obviously ill founded, enjoyed its day of fame. It was soon followed by another conjecture, more ingenious indeed, but which was not so generally attended to, as the preceding. Dr. Cole of Worcester, towards the end of last century, suggested an idea, that the proximate cause of intermitting fevers depends on a laxity or debility of the brain and origin of the nerves. The supposition is not so improbable; but the superstructure, which the author has raised, is absurd, and unsupported either by fact or probability. Yet, if ~~except we except~~ Mundy, an author who offered a conjecture of a similar kind, in a work entitled *Βιοχημολογία*, Borelli and Cole are the only writers prior to the time of Hoffman, who considered the nervous system, as directly affording a seat for the proximate cause of fevers. Hoffman, whom I have just mentioned, was a celebrated professor at Halle in Saxony. He flourished in the earlier part of the present century,

published many volumes, and certainly possesses the merit of having enlarged our views on the subject of fevers. His theory of the proximate cause is not only more ingenious, but certainly has more appearance of truth, than any other, which had been offered to the public at the time it appeared. The *cause* ~~cure~~ of fever consists, in his opinion, in a spasmodic affection of the nervous system. It is a truth which few people will attempt to deny, that a spasmodic stricture of the surface of the body generally takes place in ordinary cases of fever; yet we must perhaps also acknowledge with Dr. Cullen, that a spasmodic stricture is not certainly and uniformly the first essential part of a febrile disease. Some other thing is frequently observed to precede the spasm, which, in the opinion of the last mentioned celebrated professor, has a right to be considered as a proximate and essential cause. But as the theory of the proximate cause, assigned by Dr. Cullen, is not only more plausible and complete than any preceding one; but still increasing in popularity and fame, it will not be superfluous, if we stop to examine it with more attention. The remote causes of fever, according to this author, are sedative powers, applied to the nervous system, which diminishing the energy of the brain, thereby produce a debility in the whole of the functions, and particularly in the action of the extreme vessels. Such, however, is at the same time the nature of the animal economy, that this debility proves an indirect stimulus to the sanguiferous system; whence by the intervention of the cold stage and spasm connected with it, the action of the heart and larger arteries is increased, and continues to be so, till it has had the effect of restoring the energy of the brain, of exciting this energy to the extreme vessels, of restoring therefore their action; and thereby specially overcoming the spasm affecting them: upon the removing of which, the excretion of sweat, and other marks of

relaxation of excretories take place. This theory of fever holds out an appearance of great simplicity, and of perfect connexion. I wish we could say that it had an equal claim to truth: but I am afraid it will be found, on a careful examination, to be no more in reality than an ingenious hypothesis, the leading principles of which can scarcely be proved even to exist. I do not pretend to enter deeply into the discussion of the subject; yet I cannot avoid representing, in a few words, some circumstances of difficulty in this author's theory, which are not easily reconcileable, either with reason or observation. It might be doubted, in the first place, if the remote causes of fever are actually of a simple sedative nature; but at present I shall admit that the first principle, which is assumed by the professor, is in reality a fact, and proceed to enquire, if the rest of the doctrine is capable of being defended, even on this foundation. It constitutes the sum of Dr. Cullen's theory, as was mentioned before, that the remote causes of fever occasion debility, or diminished energy of the brain and nervous system; that this debility necessarily gives rise to spasm, and increased action of the heart and arteries; which continuing for a certain length of time, finally removes the disease. Thus the different stages of fever appear to follow each other as cause and effect; and debility in the first instance, is supposed necessarily to give rise to reaction. Such a supposition is not very obvious to reason, and has not much support from the analogy of facts. It would be easy to mention examples, where the application of debilitating causes as is not observed to be followed by obvious reaction of the system; but at present I shall content myself with the familiar one of the application of cold. It is perfectly well known, that cold, when constantly and uniformly applied to the body, even goes so far as absolutely to extinguish the powers of life, in a part, or in the whole, without

our being able to perceive any efforts on the part of nature to stop the progress of this destructive tendency. From this we may fairly infer, that common debilitating causes, at least while they continue to be applied in the same constant and uniform manner, do not necessarily excite the reaction of the system: but I will even go farther, and venture to affirm, that spasm and reaction do not necessarily follow very great degrees of debility, which appear to proceed from the presence of a febrile cause. During the time I remained in America, I had frequent opportunities of witnessing the truth of this assertion. In the southern provinces of that country, particularly in the summer and autumnal months, the intermitting fever was generally epidemic in a high degree; but its general cause, which was then so abounding in the atmosphere, often injured the actions of life, without producing a regular train of operation; that is, one part of the disease appeared without that mode of action, which is supposed, by our author, to be its necessary effect. Thus, I have seen the most extreme degrees of debility and languor in all the functions, continue even for eight or ten days, without our being able to discover the smallest marks of spasm, or obvious reaction. This inactivity and languor sometimes vanished suddenly; and the body resumed its ordinary health and vigour, frequently without an evident cause. On the next day, however, or perhaps the day following it, the patient was surprized with a regular paroxysm of fever. From this it appears very plainly, that if the immediate cause of fever actually consists in debility, this debility necessarily undergoes a peculiar, but hitherto undefined species of modification, before it can be considered as the cause of the subsequent parts of the disease; a concession which leaves us perfectly in our former state of uncertainty and ignorance.

As it may be concluded from the facts, which I

have mentioned, that spasm and reaction are not the necessary consequences of the application of debilitating causes, either common or febrile; so if we pursue our author's train of reasoning farther, we shall not find his inductions to be very consistent, or very convincing. If we are disposed to grant, that the remote causes of fever actually diminish the energy of the brain, it is not an obvious inference, that the circumstances of this diminished energy have the certain effect of exciting the reaction of the system. It appears, in short, like ascribing rest and motion to the same power. But to smooth the prominent features of this apparent inconsistency, the ingenious author has thought fit to assume a principle, the existence of which is very ambiguous in its enlarged sense, and very insufficient in its limited one. Dr. Cullen does not admit of the Italian principle of *autoisateia*; he however ascribes effects to the *vis naturæ medicatrix*, which are not capable of being explained mechanically. I mentioned before, that no efforts of nature are perceived to arise, under the uniform and constant application of a debilitating cause; but though this is true, I must likewise observe, that when these debilitating powers, from any cause whatever, actually cease to act, abate materially in the intensity of their action, or suffer change in its mode, before the vital principle is irrecoverably destroyed, nature, which persists in continuing life, and even struggles in attempting to maintain it, may then be said to raise efforts, which have a tendency to restore the body to its ordinary health. This power, which to a certain degree, resists causes of a destructive tendency and which endeavours to restore to their original state the derangements of the system which have actually taken place, is only a limited degree of the *vis naturæ medicatrix*. It is in short, no more than an effort to continue the action of living; yet it is all, which we shall, at any time, perhaps, be able to perceive.

I have thus mentioned briefly some objections, to this celebrated theory of Dr. Cullen. The ingenuity of the author is acknowledged to be great; the pains and labour, which he has bestowed in completing his favourite doctrine appear likewise to be considerable; yet I cannot help remarking, that its defects are still so obvious, that we are unavoidably obliged to be satisfied with one of these conclusions: viz. either that the debility, which is supposed to be the cause of fever, is of a peculiar but unknown kind; that it ceases to act, or changes its mode of action from an accidental cause, or from something in its own nature; or that a reaction arises in the system, from a principle of consciousness of the destructive tendency of this debilitating power. It is not consonant with the common laws of the animal economy; that reaction should arise in that part of the system, where the debilitating influence has been primarily and principally exerted. Suppositions of this nature, are only subterfuges, and no more in reality than mysterious ways of acknowledging ignorance.

The opinions I have enumerated above are the principal ones, which have been advanced by medical writers, on the subject of the proximate cause of fevers. Though numerous, they are all reducible to two general classes; viz. either to conjectures, which are totally without foundation; or to circumstances, which are in fact only symptoms or parts of the disease, some of which are more, others less essential. The proximate cause of fever, is a certain peculiar state of the body, on which the disease, or the subsequent parts of the disease, necessarily depend. It is, in short, the first essential action of the febrile cause; but this action is so intricate and difficult to be discovered, that physicians have sought for it in vain for more than two thousand years. The ancients were satisfied with the idea of preternatural heat, excited in the heart, and communicated, by

means of the blood and spirits, to the rest of the body. Hoffman, making a bolder step, introduces a spasmodic affection of the moving fibres; and Dr. Cullen, going still farther, lays the principal stress upon languor and debility, or weakened action of the nervous energy. Increased heat, spasmodic stricture and marks of debility are generally present, in various degrees, in the different stages of fever; but debility for the most part precedes the others; and on this account, if equally essential, has a preferable right to be considered as the first part of the disease. There is still reason to doubt if it actually is the first. I have myself attended carefully to the manner in which intermitting fevers approach. The first thing which I observed in others, or what is still more to be depended upon, the first thing I felt in myself, was usually a disagreeable, but a peculiar affection of the stomach. The precise nature of this affection I am unable to define in words; but I knew it so well by experience, that I always considered it as a warning, (and it was sometimes the only warning, which I had,) of the approach of the paroxysm. It was often accompanied by flatulence, and it sometimes preceded the first feelings of the languor and debility, nearly the space of an hour. The observation of this fact has occurred to me frequently; and I cannot avoid concluding, that it gives room to believe, that the debility, which is so commonly the fore-runner of fever, instead of being the first and principal mode of action of the febrile cause, is only a part of that action, —perhaps not the most essential. As Hippocrates appears at a very early period to have been perfectly sensible, that something besides a simple increase of heat was necessary to constitute a fever; so we may now perhaps conclude, with equal reason, that debility has some other circumstances combined with it, which we have not been able to discern very clearly.

Having said that there are not any of the numerous theories, which have as yet been offered to the public on this important subject, in any degree satisfactory, it might be expected, perhaps, that I had something of my own to bring forward, which might be more perfect, at least in my own opinion: but I willingly acknowledge, that I have no such pretensions. After fourteen or fifteen years of careful observation, and daily reflexion on the phenomena of fevers, I am obliged to confess, that my opinion still remains to be formed. The proximate cause of this disease, is a subject of a dark nature. It is such, perhaps, as our limited capacities will never develope. But though we despair of ever attaining clear ideas of its specific nature, there are still some useful circumstances connected with it, which we comprehend with clearness. We know, that the more general remote causes of fevers, are certain invisible exhalations, sometimes more evidently arising from marshy grounds, sometimes more obscurely diffused in the air, and sometimes obviously proceeding from the bodies of our fellow creatures. We know, likewise, that these causes which are unfriendly to the human constitution, are variously modified and combined, and of various degrees of force or in various states of concentration; but we proceed no farther with certainty. We are not able to ascertain the nature of these effluvia, and it is only by conjecture that we trace them in the channels by which they enter the body. The changes which they operate on the solids, fluids, or nervous system, before their action becomes obvious, are totally unknown to us. We know, though the body lie exposed to exhalation, even in a concentrated state, that an appearance of disease is not, generally, the instantaneous consequence. A space of time intervenes, various indeed according to circumstances, but always such as gives room to believe, that the cause requires, and actually under-

goes a modification, before it is capable of producing a fever, or the paroxysm of a fever. The circumstances connected with the approach of fevers, particularly intermitting fevers, afford an illustration of my meaning. The cause of the disease, so far from producing the fever immediately when applied to the body, often lurks for a considerable time in the constitution, without perceptibly injuring the ordinary actions of life. Sometimes it gives rise to affections, which are apparently very different from their real nature. Thus a person often languishes for days, weeks, or even longer. The indisposition suddenly and unexpectedly vanishes: and the apparent recovery of health is soon followed by a paroxysm of regular fever. In other cases again, the attack of the disease is sudden; and its formation from the beginning distinct. This fact affords a presumption, that, in consequence of a particular modification, which is only accomplished in a certain space of time, but the nature of which we do not in the least comprehend, an aptitude is regenerated between the remote cause of the disease, and the relative state of the body. When the state of the body, and the remote cause approach to, or arrive at a state of mutual correspondence, the disease is produced. When this state, which I call an aptitude, is changed or destroyed, the disease vanishes, or suffers a change of form. This is a fact, which cannot be disputed; and it seems to be the extent of our knowledge, on the important subject of the proximate cause of fevers.

C H A P. VI.

OF THE GENERAL HISTORY OF THE FEVER OF
JAMAICA.

IT will not be improper to remark, before I begin to describe the history of this fever, that though the endemic which prevailed at Savanna la Mar, notwithstanding much variety of forms and symptoms, is considered as only one and the same disease, yet it may also be observed, that the variety of these appearances is sometimes so great and remarkable as to occasion considerable perplexity to the practitioner. The symptoms and form of this endemic appeared, on a superficial view to be constantly varying; yet by attending more closely to the course, progress, and changes of the disease, these apparent irregularities vanished gradually, the varieties being in fact only accidental, and often depending on very trivial causes. The analysis of the different cases of fever, which came under my care, during the time that I lived in Jamaica, furnished me with this information. I formerly mentioned the manner in which my observations were conducted: I have only now to add, that I trust the method, which I adopted, has enabled me to give a more accurate history of the fever of Jamaica, and to explain more satisfactorily than has been done hitherto the various sources of the many irregularities which are observed to occur. I am perfectly sensible that my experience has been too limited, to give me hopes of rendering the present work complete; yet I would flatter myself, that it will not be altogether useless: I totally disclaim theoretical opinions, and content myself with a plain narrative of facts; neither do I aspire to any higher praise, than care in observing

the appearances of the disease, and truth in relating the appearances which I have seen.

Before proceeding to give a particular history of the different varieties of the endemic fever of Jamaica, it will not be improper to mention the more general symptoms, which distinguish the disease, and to trace an outline of the course, which it has been commonly observed to pursue: and I may remark in the first place, that though debility is usually considered as the first sign of an approaching fever; yet, if we attend minutely to all the circumstances of invasion, it will not generally be difficult to perceive, that a disagreeable, though undescribable affection of the stomach, takes place previous to the smallest perceptions of languor or debility, which are commonly only immediate fore-runners of coldness and shivering. This coldness, which was observed to be various in duration, as well as in degree of force in the fevers of this country, was succeeded by flushings of heat alternating with the cold, and increasing gradually till the heat at last prevailed. The hot fit, which was likewise of various duration and of various force, had many new symptoms joined with it, the principal of which were such as shewed an increased circulation, or an irregular determination of the blood to the different parts of the body. This hot fit, and the disturbances connected with it, according to circumstances, continued for a longer or shorter space of time; at last sweat breaking out on the head and breast, extended itself gradually to the extremities, and accomplished after a certain continuance, either a total remission of the fever, or a considerable abatement of the violence of the symptoms. It is almost unnecessary to mention, that this remission or abatement of symptoms was of longer or shorter duration, and more or less complete in fevers of different forms. An aggravation of symptoms succeeded to the re-

mission; but it was usually observed to begin without preceding coldness, and frequently without marks of preceding languor or debility. The hot fit now ran high, and all the symptoms were frequently more violent than they had been observed to be in the first paroxysm. Sweat at last made its appearance, followed in most cases by a remission, less perfect indeed, than the preceding one, but still distinct enough to be clearly traced. In this manner things went on for a longer or shorter space of time, the paroxysms usually increasing in violence, and the remissions becoming sometimes more, though in general less perfect, as the disease advanced in its progress. I may further observe, that there was occasionally a change of the type, sometimes a change of the nature of the symptoms in the course of the illness; and that, where either of these were the case, the disease was usually of longer continuance; at the same time, that the order of the critical days was disturbed in consequence of these changes.

The resemblances, which I have mentioned above, were found in all the different species of the remitting fever of Jamaica; but from causes, which were not always perceived, and which sometimes appeared to be very accidental, the disease was distinguished in a part, or in the whole of its course, by the prevalence of a train of symptoms of such a particular nature, as gave occasion to the distinctions of inflammatory, nervous, malignant, putrid or bilious; the separate histories of which I shall now relate more circumstantially.

SECTION I.

OF FEVER DISTINGUISHED BY SYMPTOMS OF INFLAMMATORY DIATHESIS.

THE variety of fever, which I shall describe first, is that, where the inflammatory diathesis prevailed in different degrees. Where this diathesis was moderate, the disease was usually of the least complicated form, as well as of the least dangerous nature, of any of the fevers of Jamaica. The paroxysms were generally regular, and complete in all their parts, and terminated, for the most part, by a copious sweat, in a perfect remission: the pulse was full, strong, and regular; without uncommon hardness or tension; whilst the heat of the skin, though sometimes great in degree, was generally free from that burning pungency, so common in some other species of fever. It was less removed, in short, from a simple increase of the natural warmth. I may further remark, where this moderate degree of inflammatory diathesis characterized the genius of the disease, that the danger was seldom great; and that the termination or crisis was generally regular and final. But though this degree of the inflammatory diathesis was frequently observed to be a sign of safety, and of regular crisis: yet it also often happened, where the diathesis prevailed in excess, that the symptoms of excitement ran unusually high, and that a serious danger threatened life. The pulse, in such cases, was not only frequent during the paroxysm, but it was likewise, quick, hard, and vibrating; the heat was often intense; the internal functions and the various secretions, were considerably disordered; at the same time, that a very obstinate spasmodic stricture prevailed on the surface of the body. The remission which followed, for the most part, was obscure; the

pulse frequently retaining a preternatural quickness and hardness; whilst there was generally a considerable degree of febrile heat on the skin.

The symptoms, which I have just now mentioned indicate different degrees of the real inflammatory diathesis; but besides actual symptoms of real inflammatory diathesis, there were likewise found fevers, with the appearances of a similar disposition, though the real genius of the disease was in reality of a different nature. It is of importance in practice to distinguish those ambiguous appearances; but it is not always easy to do it with certainty. We may remark, however, that the apparent inflammatory diathesis was usually accompanied with marks of great irritability, and sometimes with marks of violent excitement during the paroxysms; while languor and great depression of spirit were frequently perceived to attend the remissions. The pulse, which at one time was hard, irregular, and quick, at another was frequent and low, and sunk under a small degree of pressure. The heat of the body was not always great, yet it was pungent,—and left a disagreeable sensation on the hand: the secretions were often irregular; the countenance was confused, clouded and overcast, the eye was sad, and sometimes appeared as if it were inflamed; the feelings were unpleasant to the patient himself: there was great irritability of temper; and the state of the skin impressed us with the idea, that there was a strong spasmodic stricture prevailing on the surface of the body.—The above are the principal circumstances, which were usually present in the different states of inflammatory fevers; yet these circumstances were sometimes so variously complicated and combined with others, that the accurate discrimination of them must be left, in most cases, to the observation of the individual himself.

Those different states and degrees of the inflammatory diathesis, which I have described above, were

sometimes general throughout the whole of the body, not affecting one member more remarkably than another; sometimes they were partial or seemed to be connected with a principal affection of a particular part: and where this was the case, the local affection, and the general diathesis of the system, usually had a mutual correspondence with each other. Thus, where the inflammation affected the substance of the liver or lungs, the general inflammatory diathesis was usually in a moderate degree; while the highest excess of general vascular excitement often accompanied inflammations of the membranes of those organs. But though inflammation of membranes was often accompanied with a high degree of general inflammatory diathesis; yet there were likewise some kinds of those local inflammations which communicated only a low, or an ambiguous degree of their diathesis to the general system: such are some of those inflammations, which occasionally affect the surface of the alimentary canal, and which appear, in general, to be of the erysipelatic kind.

SECTION II.

OF FEVER WITH SYMPTOMS OF NERVOUS AFFECTION.

THE endemic fever of Jamaica, was not oftener distinguished by symptoms of general inflammatory diathesis, than by circumstances of nervous affection. The beginning of this form of the disease, was often characterized by a high degree of that disagreeable affection of the stomach, as also by much of that languor and debility, which are commonly fore-runners of fevers in general. To these symptoms succeeded a slight degree of chilliness, followed

by a hot fit, which often continued long, but seldom ran high. The pulse was small, frequent, and easily compressed. It varied with change of posture;—and sometimes was so much affected when the patient was raised upright, as totally to disappear; the heat of the body was seldom great; the secretions and exertions were generally irregular, and the internal functions were much disordered. The mind was usually affected, affected however in various degrees, and in various ways. Sometimes there was a lively delirium, sometimes the delirium was low and desponding; and, as the one or other of these was the case, the appearance of the eye and countenance was chearful or sad. The tongue was sometimes moist, sometimes dry, but seldom very foul; thirst was irregular, nausea was frequent, and the state of the stomach was generally very irritable. There was likewise, in most cases, deep and heavy sighing, and, unless in times of preternatural excitement, a very uncommon degree of despondency. The above were the principal symptoms of the nervous fever of Jamaica. The paroxysms in this disease seldom exceeded twelve hours in duration; while the termination or abatement, was usually distinguished by sweating, though seldom by such sweatings as extended completely to every part of the body. The remissions were not by any means perfect: the head-ach, and other disagreeable feelings usually abated; but signs of languor still continued, and marks of spasmodic stricture for the most part remained on the surface of the skin. I may further observe, that as the paroxysms generally increased in violence, in the progress of the fever; so it was very seldom that the remissions put on an appearance of greater distinctness, as the disease approached to its termination.

Such is the general history and the progress of the disease, which might be distinguished by the name of the nervous fever of Jamaica: but besides those cir-

cumstances, which I have mentioned above, others were sometimes found attending it, which, though less regular and constant, deserve still to be taken notice of. Thus the first stage of the paroxysm, instead of the more usual appearances, was occasionally distinguished by fits, which appeared to be of the epileptic kind. These fits in some cases were succeeded by a lively delirium, in others by stupor or insensibility. The delirium, which was a common symptom of this disease, ran high in several instances; though it more generally amounted only to an absence of thought, or difficulty of recollection. It is a circumstance of some curiosity likewise, that instead of a paroxysm, consisting of different parts in a certain order of succession, there was sometimes a total stupor and insensibility, which continued for a determinate space of time, without even being succeeded by obvious marks of fever: whilst the time of the paroxysm, in other cases, was distinguished by such a degree of tremor and mobility, as nearly approached to the disease known by the name of St. Vitus's dance. And further, besides these strange and irregular appearances, spasms and excruciating pains in different parts of the body, in many instances, were the leading, indeed almost the only symptoms of the disease.

It is not only curious, but it is indispensably necessary in the conduct of our practice, to observe with attention the various modes of action of the cause of fevers, and to estimate with precision the various combinations. The cause of fevers, in exerting its principal action on the nervous system, sometimes produces excitement, sometimes occasions depression; effects opposite to each other in their nature. Excitement and depression are two general and opposite modes of action; yet besides these we often observe others, which do not belong wholly to the one or the other, but which seem to be compounded of both, in a manner we do not very well comprehend. This

cause of fever likewise, which acts in directly opposite ways, appears also to exert its action more powerfully at different times on one part of the system than on another; that is, it acts sometimes more immediately on the brain, or reasoning faculty, sometimes more directly on the nerves, or moving powers of the body. It may even be observed further, that all these modes of action, which are preserved distinct at one time, are combined in various degrees at another. Thus, where the cause of fever acts by producing excitement, lively delirium in various degrees is the consequence; while languor, stupor, and insensibility naturally follow the opposite mode of action. Low delirium, tremors, startings, &c. are probably owing to a compound effect. Both modes of action succeed each other rapidly; or perhaps both modes are actually present at the same time, though probably in different degrees, in the different portions of the brain. This fact at least is certain, that obvious depression is often combined with signs of great irritability. It is a remark likewise of considerable importance, that the natural functions are less disordered, where the cause of the disease acts upon the nervous system internally, or principally disturbs the intellectual powers, than where this action is obviously external: the pulse is then more regular, though often obscure; the disposition to faint is not so great; muscular mobility is less remarkable, and local pains are felt less acutely. On the contrary, where this cause acts externally, or chiefly affects the moving powers, the disposition to faint in changing posture is more remarkable; tremors, startings, &c. are more common; appearances, in short, are more fluctuating and often more alarming.

It is a matter not less useful than curious to distinguish the different species of delirium in fevers, to trace the different combinations, and to mark the apparently trivial causes, which excite, or which some-

times remove those derangements of the reasoning faculty. It is a remark, which has been often made, that while one delirious person in fever appears only to be in better spirits than usual, another, or perhaps the same person in another paroxysm of the same disease, is outrageous or perfectly furious. A third is low and languid, absent and inattentive, or, with a fixed look of vacancy, does not seem to be otherwise deranged, than by requiring greater time to recollect himself. To which we may add, that there are some, who talk coolly on things in general; but who cannot bear mention of some particular subjects.

SECTION III.

OF FEVER IN WHICH ARE DISCOVERED SIGNS OF MALIGNITY.

THE fever of Jamaica, as distinguished by signs of inflammatory diathesis, or by circumstances of nervous affection, prevailed principally at Savanna la Mar; yet besides the above forms of the disease, there sometimes likewise occurred others, which shewed marks of peculiar malignity. It is difficult to define precisely in words the character of the disease, which I now attempt to describe; its discriminating marks, not consisting so much in one or two symptoms, as in a certain assemblage of circumstances residing chiefly in the state of the eye and countenance of the patient, and conveyed with difficulty in verbal description. I may remark, in the first place, that there was seldom any thing very particular in the manner of invasion of this species of disease. The cold fit was rarely violent in degree, though it was often of long continuance: neither did the hot

fit usually run high, in the common acceptation of the word, though it was sometimes attended with circumstances peculiarly disagreeable. The pulse varied remarkably. It was obscure, or scarcely to be felt in some; in others it was strong, though unequally so; the artery, in many instances, being hard and contracted, with a peculiar vibration in the stroke. After these symptoms and others, which are usual in this stage of fever, had continued for a longer or shorter time, sweat began to make its appearance on the head and breast, which extending itself gradually to every part of the body, was at last followed by a remission, tolerably perfect for the most part, though there still remained some strange and unpleasant sensations. It does not appear that there is any thing very uncommon in the symptoms, which I have hitherto taken notice of: those which follow are more characteristic. The state of the eye and countenance, afford the surest signs of the malignity of the disease; but there is difficulty in discriminating those appearances. The face is not unusually flushed in fevers; but, in the present case, the countenance exhibits something else besides an appearance of simple flushing. It is likewise grim, dark and overcast, with such marks of confusion and distress, as if the patient were agitated by some resentful passion. The eye is sad and desponding; and the whole appearance, in short, indicates such a state of mind, as we should be disposed to style malignant. It is in such a state of the countenance as I have described, that the character of this species of fever chiefly resides; yet besides this, some other circumstances frequently attend the disease, which are less usual in ordinary fevers. The paroxysm for instance returned, for the most part, much sooner than the regular period, always with greater violence, and sometimes with new and alarming symptoms. It declined in twelve or fourteen hours; but the remission was less perfect than the preceding one; the next re-

turn of fever, which was likewise much earlier than the stated hour, was often ushered in by convulsions, and the time of it occupied by stupor or coma. The tongue was likewise irregularly moist or dry. If dry, it was generally covered with a black scurf; if moist, with a thin glutinous coat, through which the red surface shining obscurely, presented an appearance of a leaden colour. In this case the mouth abounded with a ropy saliva. But besides the above symptoms, there were also violent twitchings in the stomach and bowels, sudden squeamishness, faintness, anxiety, restlessness, frightful dreams, distressing apprehensions, and frequently after the second paroxysm, a particular crowded eruption (not unlike iron-burnt blisters,) on the upper lip, which for the most part spread towards the nose. The type of this fever, it may be further remarked, was usually of the single tertian kind, generally anticipating by long anticipations. In most instances this malignant disposition was discoverable at the very beginning; yet in others, no symptoms of a doubtful nature made their appearance till after the third revolution.

SECTION IV.

OF FEVERS IN WHICH ARE OBSERVED SYMPTOMS OF A PUTRESCENT TENDENCY.

WE meet with the term putrid fever, or fever with putrescent tendency, in the writings of almost every author who has treated of the diseases of hot climates: but though this expression is so much the common language of practitioners, I cannot help observing, that a remitting fever, with symptoms of a specific putrefaction, did not once occur to my

observation in the island of Jamaica, during the time that I lived in that country. I must however add, that though a remitting fever specifically putrid is actually a rare disease; yet I do not attempt to deny, that a putrescent tendency is frequently present in the primæ viæ, in a very considerable degree; and that marks of it are sometimes discoverable, even in the general system, at a late period of the illness, when the vigour of life has abated, and the powers of circulation have begun to fail. This however is so accidental and unessential, that it is only in compliance with the general language of medical people, that I think it necessary to describe a disease, where these symptoms are observed to prevail. The tendency to putrefaction, which was observed in the fever of Jamaica, sometimes begins in the primæ viæ; and from the primæ viæ was communicated to the rest of the system; sometimes it remained confined to the limits of the intestinal canal, throughout the whole duration of the disorder; in which case flatulence, ructus, anxiety, nausea and thirst were the symptoms which were chiefly troublesome: the belly likewise was generally loose, at the same time that the stools were dark and fetid. But where this tendency was communicated from the primæ viæ to the rest of the body, or otherwise made its appearance in the general system, a form of disease arose distinguished by the following symptoms. If the tendency to putrefaction appeared at an early period, the heat of the skin made a more disagreeable impression on the hand, than was usual in some other fevers; the skin itself was likewise for the most part, dry and constricted; the thirst was irregular, sometimes intense, sometimes from local affection of the fauces, apparently little increased.— The appearance of the eye was often sad; sometimes it glistened with unusual brilliancy; sometimes it seemed to be inflamed. The countenance was generally flushed, often particularly confused, and of a grim

and clouded aspect. I have however frequently observed, where symptoms of putrescency discovered themselves at a late period of a fever, the preceding course of which had been distinguished by circumstances of nervous affection, that the bloom of the complexion was uncommonly fine and delicate. To the above symptoms might be added, great irritability of temper, general uneasiness of sensation, and disorder in all the functions of the body. When the fever assumed this appearance, paroxysms and remissions were generally obscure and irregular. The fever indeed often subsided in a small degree; but the future remissions generally became less distinct, as the disease proceeded in its course. The tongue assumed different appearances, at different periods and in different persons. In some it was moist, in others parched and dry. It was not universally foul, at least it frequently happened, that the edges were clear and beautifully red in their colour. The lips likewise were sometimes smooth, and of a cherry-like appearance; at the same time that the gums were inflamed and spongy, as they usually are in scurvy: the pulse likewise was small for the most part; but it was irregularly so. I say nothing of the disposition to faint in erect posture, which though generally enumerated among the signs of putrid fevers by authors, does not in fact appear to constitute a criterion of the disease.

SECTION V.

OF FEVERS ACCOMPANIED WITH AN INCREASED
SECRETION OF BILE.

THOSE species of fevers, which I have mentioned above, seem to affect the general system, or every part of the body nearly alike; but besides these, we sometimes meet with others, which are distinguished by local affections, or increased determinations to particular parts in a degree so remarkable, as to personate very exactly a peripneumony, a hepatitis, or inflammation of the bowels; the accompanying fever being at the same time so slight, as scarcely to be considered as a primary affection. As an accident similar to these local affections of the liver or lungs, we may reckon an increased secretion of bile. The cause of fever, from circumstances which we do not always perceive, sometimes acts with particular violence on the biliary system, in consequence of which the secretion of bile being preternaturally increased, a disease arises, which without much impropriety may be called bilious. But though this irregular action of the morbid cause, on the biliary system, frequently gives rise to bilious appearances in the fevers of Jamaica; yet these appearances are in fact often owing to causes more accidental, and more remote than even this. Nausea and vomiting are among the common symptoms of fevers in every country; but they are particularly frequent in those of the West-Indies. It is well known that a continuance of nausea, or that a repetition of the action of vomiting, increases the determination, not only to the stomach, but likewise to the parts which are near to it. Hence the secretion of bile is preternaturally increased secondarily by the ordinary effect of vomiting, and bilious appearances become a

necessary consequence of this accidental symptom of the disease. In those two manners, viz. in consequence of the irregular action of the morbid cause on the immediately biliary system, or from a secondary effect in consequence of its action on the stomach, the bilious fever may, in some respects, be considered as a disease of nature; but besides this, it often originates from our own treatment, viz. from the repeated use of emetics, or of cathartics, which are violent in their operation. The accidental appearance of bilious vomitings, in the fevers of hot climates, furnished medical authors with a pretence of forming a new theory, and of directing the mode of practice to a particular view. Influenced by this appearance, they assume it as a fact, that a vitiated quality, or a redundant quantity of bile constitutes the essential cause of the disease; and on this foundation adopt the plan of repeated evacuating, both upwards and downwards; a practice which evidently increases the secretion of the bile. Hence, a disease, or the symptom of a disease, arises wholly from this mode of treatment; and the removal or cure of it is afterwards attempted by a perseverance in the means, which originally gave rise to it:—of this I have seen numerous examples.

I have now described the remitting fever of Jamaica, as characterised by symptoms of a different appearance. I may further remark, that where these symptoms were unmixed with each other, there was little difficulty in the distinction, and little embarrassment in planning or executing the indications of cure: but it sometimes also happened, that the different species, which I have described separately, was so perplexed and complicated, that it appeared uncertain to which kind the disease properly belonged; or to which view the practice ought to be principally directed. Symptoms of putrescency, for instance, were often combined with symptoms of apparent inflam-

matory diathesis; as fevers with nervous affection, or putrescent tendency, were sometimes accompanied with marks of peculiar malignity. It happened often likewise, that the nature of the disease suffered a total change after a certain duration; or that a fever with one train of symptoms ceased, whilst another with a different train began.

It would be a matter of no small importance, were we able to ascertain the various causes, which influence the various appearances of the same disease; but this knowledge is not easily attained:—much of it indeed lies beyond the reach of our comprehension. We may however remark, that the season of the year usually has some effect on the diathesis of the system, and often on the type and form of the fever. Thus, in the dry season, though the remissions are not always more perfect, the type is commonly more simple, and the general diathesis is oftener inflammatory. In the rainy months, on the contrary, remissions are more perceivable, but the type is more complicated, and the general diathesis of the system has a stronger tendency to putrescency, often with a mixture of symptoms of nervous affection, sometimes with symptoms of a malignant nature. The stomach, bowels, and biliary system likewise suffer more in this season than in the drier months of the year. But besides this difference, which arises from season, we also find very constant effects from local situation. Thus in hilly countries there is generally more of the inflammatory diathesis, with more frequent determination to the head and lungs, and less obvious remissions, than in flat and champaign countries, where the stomach and biliary system suffer in a more peculiar manner.

C H A P. VII.

OF PROGNOSTIC IN THE FEVERS OF JAMAICA.

TO be able to perceive at a distance, the approach of danger or returning health, is a knowledge highly satisfactory and useful to the physician; but it is a knowledge which is not easily attained: for to judge with certainty of the event of fevers, requires not only long and attentive observation, but a discrimination of complicated and ambiguous appearances, which does not depend always upon attention alone. The sagacious Hippocrates is generally considered as the first, who laid the foundation of the science of prognostic; and we certainly must allow, that he has left us many important and valuable observations on the subject; yet we may also add, that his decisions in many instances, are precipitate. Hippocrates seems generally to have placed too great confidence in signs separately considered, and to have formed his conclusions too often on the authority of single facts. Thus he has sometimes considered as fatal in themselves those signs, which in reality are only dangerous. The absolutely fatal signs in fevers are actually few in number. I am able to affirm, from my own experience, that people are sometimes restored to health after many of the usually reputed fore-runners of death are present. We have, in fact, as yet only an imperfect knowledge of prognostic in fevers; but the field is still open, and careful observation, it is to be hoped, may enable us in time to supply the defects. I dare not venture to assert, that I have advanced beyond others in this necessary and difficult science; but I am disposed to flatter myself, that the following attempt to appreciate the marks of danger or safety in the fevers of Jamaica, may be

found in some degree useful. It contains the result of my own observations in that country; and though I am perfectly conscious that the rules are often defective, yet I likewise know, that I have suggested some hints which have not been commonly observed, and which may help to direct those, who have not had much experience of their own.

Prognostic is such, as applies to fevers in general, or more particularly to the different species of the disease. The type or form, the general course and tenor of the disorders, and the *general* nature of the paroxysms often afford useful information. From the type alone, we do not often obtain much that is to be depended upon. Long and distinct intermissions are commonly accounted signs of safety; yet we frequently see instances of the single tertian proving fatal, while types of greater complication are often void of danger: in general, however, complicated types are suspicious—and perhaps more commonly fatal than others. But though a knowledge of the type of the fever abstractedly considered, does not commonly afford any material indication of danger or safety, yet the time of the return of the paroxysm is a subject, from which more may be learned. An anticipation of an hour or two, is seldom much to be regarded; yet an anticipation of ten or twelve is always suspicious. It either indicates a latent malignity, or a tendency in the disease to change to a continued form. The complication of another fever, or the doubling of type is by no means favourable; yet it is much less to be dreaded, than a long and an irregular anticipation. Anticipations have been generally considered as signs of the increasing force of the fever; so the type which postpones, is usually believed to indicate a disease, which is hastening to a favourable termination: the effect however is sometimes the contrary. I have myself seen some instances, where, in consequence perhaps of weakness and im-

paired sensibility, the return of the fatal paroxysm, though it probably had commenced sooner, was not clearly perceived till after the usual hour of attack. But besides those indications of danger or safety, which may be drawn from the nature of the simple type, or from the hour of return of the paroxysm, the state of the paroxysms and remissions deserves likewise to be attended to. It was generally observed where the paroxysms were regular, and assumed a completer form in the progress of the disease, that there was not generally much reason to dread an unfavourable event. Hopes of safety might likewise be entertained with still greater confidence, where the paroxysms, though more violent in degree, became more regular and distinct after the use of bark, wine and stimulants. On the contrary, it was always an indication of danger, where they became longer or lost the distinctness and regularity of their form, as the disease advanced in its progress. Changes from bad to good, in the course of the fever, also indicated more safety as the opposite changes indicated more danger, than if circumstances equally unfavourable had continued from the beginning.

In enumerating those particular signs or symptoms, from which we are led to form a judgment of the event of the remitting fever of Jamaica, I shall consider in the first place the state of the pulse. The pulse is so differently affected by the same causes in different people, and individually subject to so many peculiarities, that conclusions formed solely upon this basis must ever be fallacious. Hippocrates, who has treated very fully of the other signs of prognostic, is totally silent on the subject of the pulse. He has mentioned the term, indeed, in several parts of his works; but it does not appear, that he had a perfect knowledge of the nature and indications of the pulsations of the arteries. The subject was somewhat better understood before the time of Celsus: yet this

author does not believe, that any information could be drawn from the state of the pulse alone, which was in any great degree to be depended upon. Galen, who is generally diffuse on every subject, has treated very fully of the nature of the pulse. He has indeed multiplied distinctions to an amazing extent, and suggested combinations of endless variety; yet notwithstanding this apparent minuteness, there are still several important observations with respect to it, which have escaped him altogether. It is not many years ago, that Dr. Solano, a Spanish physician who practised at Antequiera, opened some new and curious views concerning the pulse, and its various indications.

The detail of facts with which this writer has furnished us, is really wonderful, and the candour with which he has related them, independent of the testimony of several respectable authorities, engages us to give him credit. I had not heard of Solano's discoveries at the time I lived in Jamaica, and I do not find that I had ever taken notice of observations similar to those he has mentioned. I was able indeed, for the most part, to foretel from the nature of the pulse, even in the beginning of the disease, whether the fever would be of a continued or remitting form; but I did not discover any signs from it, which led me to form a judgment of the future mode of termination. I may add, that I met not with any instances of crisis by hæmorrhage; neither did I ever take notice of the rebounding pulse. The intermitting pulse occurred frequently, sometimes as a forerunner of death, sometimes as an attendant of favourable crisis: but I cannot say, that I observed that it ever presaged a future diarrhea. I shall however pass over the observations of others without further comment for the present, and content myself with relating those circumstances of pulse connected with danger or safety, as they occurred to my own observation in the

remitting fever of the West Indies. I must remark in the first place, that independent of peculiarities of constitution, a weak, a feeble and easily compressed pulse was generally a bad one: a pulse which was indistinct and small, or small and hard, particularly at a late period of the disease, or together with delirium or clammy sweats, indicated for the most part, the most extreme degree of danger. That species of pulse moreover, where the stroke was obscure, or felt with difficulty, was suspicious at all times; but it was particularly dangerous where accompanied with a wavering, a tremulous, a constantly creeping or vermicular motion in the artery. I am not certain that my meaning will be clearly understood; yet I believe that those who have once observed this tremulous and creeping pulse, will not easily forget the danger which it indicates. It often attended a fever of a malignant kind, where the nervous influence appeared in some degree, to be suspended.—But to proceed: it is an observation so well known as to render any mention of it almost superfluous, that a frequent, an irregular, a fluttering and intermitting pulse commonly indicates danger, sometimes approaching death: yet I must add, that an intermitting pulse sometimes attended the favourable crisis of a peculiar species of fever. It was observed, however, in such cases, that the pulse was not otherwise irregular, than by failing in its stroke at the end of every third or fourth pulsation, neither was it generally found to be uncommonly frequent. Some instances of this singular appearance occurred to me during the time that I remained in Jamaica: so that I was in some degree disposed to rank the intermitting pulse among the signs of favourable crisis, in a species of fever, the preceding course of which had been distinguished by symptoms of a peculiar nervous affection. When I became acquainted afterwards with the observations of Dr. Solano, I began to doubt whether the

intermission of pulse, which I had met with in the fevers of Jamaica, might not have been a sign of approaching diarrhea, which had not occurred to my notice, rather than a sign of proper crisis, as I had formerly imagined. I remained in this uncertainty till lately, that some instances of this symptom happening at the termination of fevers in this country, have helped to confirm me in the opinion which I entertained before. I found in those cases to which I allude, that the pulse intermitted after every third or fourth stroke on the day, on which I expected the crisis. The intermission of the pulse was not of such a nature as indicated approaching death; I therefore looked watchfully for a diarrhea, but no diarrhea ensued. It must be confessed, indeed, that one of the patients seemed to be much distressed with gripes and flatus; but being deprived of the power of speech we could not obtain any accurate idea of his feelings: and no evacuation actually took place, till the day following, before which time the intermission had disappeared altogether.—Besides the above, there are some other signs of pulse which have their particular indications; but they are so generally known, that it will not be necessary to enlarge on the subject. I shall therefore only observe further, that changes from better to worse in the state of the pulse, as the disease advances in its progress, are bad, while the opposite changes are favourable: yet I must likewise add, that in those cases of favourable change, it will be necessary to distinguish carefully the pulse of coma, from the pulse of returning health.—The difference is sometimes scarcely to be known, except from the attending symptoms.

Next to the state of the pulse, I shall mention those appearances of the tongue, which, together with other concomitant circumstances, frequently afford signs of the mildness or malignity of the disease. Though we do not expect that the tongue should be

of a healthy aspect, during the continuance of a fever; yet where it is dry only in a moderate degree, or where it is covered with a smooth and whitish coat, the disease for the most part, is void of malignity, though not always of danger. On the contrary, where it is immoderately dry, or dry and black, the indications of danger are great, and I may add, still greater where a white slimy and glutinous substance covers its surface. This slimy state of the tongue was often seen at an early period, and as far as my experience goes, constantly indicated malignity. To the above we may add, a sodden or parboiled appearance of the tongue, which was not of less dangerous import than the preceding. But besides those obviously unhealthy aspects of the tongue, its appearance in some instances was not different from its natural state, except in a certain lividness of colour. This was constantly suspicious, and if not fatal, was always extremely dangerous. The danger indicated by the tongue, when it is intensely dry, rough, cracked, or ulcerated is generally known; but I must not omit to mention, that when from a dry and unhealthy state, it turns moist suddenly, or assumes its natural appearance, whilst the other signs of favourable crisis did not shew themselves at the same time, a change of the mode of action of the febrile cause is indicated,—and generally a dangerous one.—I say nothing of paleness and tremor, as these symptoms only indicate certain states of general or particular debility of the nervous system.

Vomiting is another of the alarming, and sometimes of the dangerous symptoms of the fevers of the West-Indies. If this symptom continues during the remission of the fever, without material abatement, there is reason to dread its consequences; but if it vanishes or abates in a very material degree at the decline of the paroxysm, it does not deserve to be so particularly regarded. The practice however to

which it leads is often serious. Vomiting is supposed by most practitioners to indicate emetics; but the indication is fallacious, and the practice is often pernicious. During the time that I lived in Jamaica, I had frequent opportunities of seeing vomitings rendered continual by the repeated use of emetics, which before this treatment, appeared to be only accidental symptoms during the paroxysm of the disease. I therefore at last became cautious of pursuing this view, and have reason to believe, that if I did not oftener do good than others, I seldomer increased the danger. But besides the degree and frequency of the vomiting, the nature of the matters thrown up may likewise furnish indications of the danger or safety of the fever. The various kinds of bilious vomitings have been fully explained, and the danger of each has been so particularly pointed out by many writers, especially by Hippocrates, that I pass over the subject without further notice, considering it unnecessary to repeat the observations of others. I must however remark a more uncommon kind of vomiting, which sometimes happened in the fevers of Jamaica, and which I believe has hitherto escaped the notice of observers. The vomiting to which I allude in this place, is a vomiting of a clear and ropy liquor, in which are often found swimming flakes of a darker coloured mucus. This appearance was chiefly observed, where the remissions were indistinct, and the sweats partial and incomplete. It constantly afforded an indication of danger, and I seldom found that the usual remedies were effectual in restraining it. Vomitings of black and vitiated matters are commonly known to be of the most dangerous import, succeeded by obscure hickupings, they are often forerunners of death. Yet though this is generally true, I must not at the same time omit to mention, that I have seen several instances of recovery where black vomiting had prevailed for some time; and other

cases which give me room to conclude, that hickups are not constantly fatal. I take the present opportunity to remark, that hickup was sometimes only a distinguishing symptom of the disease, which increased or declined with the paroxysm; and that in other instances it attended the favourable crisis of fevers, the preceding course of which had been characterised by symptoms of nervous affection. This species of hickup was generally alarming in degree, and equally inexplicable with the intermitting pulse, which I mentioned above as sometimes attending a favourable termination. It often continued the space of twenty-four hours, in spite of all that could be done by medicine.

Next to the indications of vomiting, I shall enumerate such as may be drawn from the presence or absence of thirst. Immoderate and unquenchable thirst has always been reckoned an unfavourable symptom in fevers. It is so undoubtedly, yet I have frequently seen very extraordinary degrees of it continuing for a length of time, without particular danger. Besides the desire for liquid in general, there is often an unconquerable longing for drinks of a particular kind;—a feeling which ought always to be attended to, and frequently complied with.—The desire for cold water is sometimes ravenous.—I have known it not only satiated with safety, but even with good effects. But though this immoderate thirst is justly reckoned a bad symptom in fevers; yet an indifference for liquid, with a dry tongue, and other marks of internal heat, is still worse. It has indeed been generally considered as fatal; but here we ought to distinguish, whether it proceeds from local affection of the tongue and fauces, or from a general failure of the powers of life. In the one case it is a mortal sign, in the other it can only be said to be dangerous.

The ancients, particularly Hippocrates and his

commentator Galen, have treated so fully of the indications of evacuations downwards, that I should be able to do little more than to copy their observations. There is one species of evacuation, however, which they do not appear to have described very explicitly, and which I have often observed to be attended with great danger. This is the frequent, small and ineffective excretion, and more particularly copious stools, which resemble dirty water, especially if accompanied with tension of the hypochondria and abdomen.

Medical writers have been long accustomed to form a prognostic of the event of fevers, from pustular or scabby eruptions about the mouth: but the sign is ambiguous, and cannot be depended upon, without many limitations. I shall however relate that which has occurred to my own observation, without troubling myself about the opinions of others. And I remark in the first place, that an eruption about the corners of the mouth, and near the lips, which comes forth freely, and soon turns into a scab, particularly if it does not appear till after the third revolution of the disease, affords a general sign of safety, at least it affords a sign that the complaint has attained the height of its violence. On the contrary, an eruption which shews itself at an earlier period, which is crowded, and makes its way with difficulty, or which resembles iron-burnt blisters rather than pustules properly so called, particularly if it is on the upper lip, and spreads towards the nose, affords a general indication of danger and malignancy.—Small and imperfect eruptions likewise are frequently a sign of a tedious disease.

The state of animal heat is another of those circumstances, which may be considered as affording an indication of the nature and event of fevers. Where the heat of the body, in the remitting fever of Jamaica, was equally diffused to the extremities,

or not differing from an increased degree of natural warmth, the disease was usually mild, without particular danger or malignity; but where acrid, fiery and pungent, though perhaps not much increased in degree, danger was apprehended with reason, particularly if the warmth was not extended equally to every part of the body. In the remissions of those fevers, which were distinguished by symptoms of nervous affection, or, as is more commonly believed, putrescent tendency, the heat of the body was often several degrees below the standard of health. The symptom was alarming, but it was not in fact of much consequence. This diminution of the heat of the body, during the remission, was not by any means a rare occurrence; but besides this, there was sometimes observed a degree of coldness, during the favourable crisis of nervous fevers, of a very singular and extraordinary kind. In some instances this coldness was not inferior in degree to that of a person dying, or actually dead; yet a distinction was perceived without difficulty. It was not accompanied with marks of stricture on the surface of the body, at the same time that the pulse was generally soft, regular and full.

Next to the state of animal heat it will not be improper to consider the indications of the various kinds of sweats. The signs of a favourable sweat are commonly known. Where that excretion was fluid, warm and universal, particularly where accompanied with a soft, full and expanding pulse, calm and easy respiration, general relief from symptoms of distress, with a cheerful eye and countenance, we might in general presume on safety of the disease, often on its favourable termination. On the contrary, where the sweat was cold, clammy and partial, particularly where the pulse became or continued frequent, small and tense, with anxiety, restlessness and disturbed respiration, a circumscribed flushing, a greasy hue of the countenance, or a wild and dejected appearance

of the eye, the situation was then alarming :—death, in short, was seldom far off. There is, however, an observation with regard to this subject, which I must not omit to mention. Authors, with one consent, have considered cold sweats as certain mortal signs in fevers ; but there appear to be exceptions to this general rule. I met with several instances, while I remained in Jamaica, where universal fluid sweats, of an extraordinary degree of coldness, accompanied the crisis of the disease. I was much alarmed when this appearance first occurred to me, but my fears soon vanished, as I found that the pulse became slower and fuller, that the respiration became calm and easy, and particularly that the eye and countenance acquired such a cheerfulness and serenity, as are usual at the favourable termination of fevers.

To the signs of prognostic, which I have mentioned above, I shall add those which are indicated by the general state of the vital powers, or by the more particular affection of parts, which are of immediate importance to life. Among the first of the affections of the vital organs, we shall consider such indications as arise from a disturbed state of respiration. A frequent, a hurried and unequal respiration, (I do not speak of that which depends on primary affection of the lungs), is justly considered as a sign of a bad disease. This is more certainly the case, where accompanied with deep and heavy sighing. Frequent sighing was a common symptom in the fevers of Jamaica, where the powers of life were depressed; and though not absolutely a mortal sign, it constantly indicated danger.

Next to the state of respiration, I shall mention the state of the intellect, or reasoning faculty, which often afforded some prognostic of the event of the fever. Delirium, I observed before, was a common symptom in the remitting fever of Jamaica. Where it vanished or abated as the paroxysm declined, it was seldom

found to be of material consequence. On the contrary, where it continued during the remission undiminished in degree, it was a symptom of the most serious nature. I mentioned in a former part of this treatise, that the cause of fever appeared to act on the brain and nervous system, in two general and opposite ways; that is, by occasioning excitement or depression. Of these two modes of action, depression was the most dangerous; unless where the excitement ran uncommonly high. But though I observed, that there are only two general modes of operation, viz. excitement and depression; yet I must also add, that the modifications are numerous, and very variously combined. Among the most dangerous and alarming species of the derangements of the intellect, we might reckon a stern fullness, an unmanageable furiousness, picking the bed cloaths, tracing figures on the wall, and such other instances of perverted judgment. Stupor and suspension of the nervous influence, as we might term it, were likewise greatly to be dreaded: unless they shewed themselves only during the time of the paroxysm, they were generally fatal, more certainly so, if they followed convulsions.

As nearly connected with delirium, we shall now consider other disturbed states of the functions of the brain, viz. the states of rest and watching. We do not expect that sleep should be found and undisturbed in fevers; yet we have been accustomed to think favourably of the disease, where the patient is refreshed by it. On the contrary, total want of rest, or unrefreshing slumbers, constantly indicate danger. There is, however, an astonishing diversity of constitution in this respect, that must always be taken into the account in forming an opinion. Want of sleep was observed to give rise to delirium in some persons very speedily; others supported it for a great length of time, without any appearance of delirium or spasmodic affection. An appearance of

sleeping, without actually enjoying the comforts of sleep is well known to be a dangerous symptom; yet it is not by any means a mortal one. Anxiety and restlessness, are often referred to the stomach; but restlessness and jactitation, as depending on the state of the nervous system, were likewise frequent, and generally dangerous symptoms. Tremors of the tongue and of the hands were common appearances in fevers, with marks of nervous affection; but I have likewise met with instances, where the whole body shook, when any motion was attempted, not otherwise than it does in paralysis or chorea sancti viti. Startings and subsultus tendinum were not uncommon; and they were justly considered as indications of danger; sometimes as forerunners of convulsion. A disposition to faint, even actual fainting, was frequent in the fevers of this country. It was always dangerous, though perhaps less so, than other symptoms which were less alarming, particularly if it suffered increase and diminution with the paroxysms and remissions of the disease. But besides these symptoms, which indicate diminished energies, or irregular action of the nervous influence, we may likewise observe, that the sphincter muscles frequently lose their power of contracting, particularly in the advanced periods of fevers. Thus persons sometimes can only lie upon their back, the eyes and mouth are half open, the powers of speech and swallowing are impaired or lost, and urine and stools pass off without consciousness or against their will. It is unnecessary to remark, that these are all symptoms of the most extreme danger. If they proceed from a general and uniform diminution of the powers of life, we may justly consider them as fatal; if they are only produced by a certain mode of action of the febrile cause, and are remarkably increased during the paroxysm, or aggravated by peculiar circumstances of constitution, we shall find many instances of re-

covery. Thus, I have often seen people recover, who could neither speak nor swallow ; who did not appear even to distinguish objects, and who were unable to retain their urine and stools ; or who were not conscious when they passed ; yet I do not pretend to have met with any of these fortunate events, where these alarming symptoms were the consequence of uniform diminution, or general extinction of the vital principle.

From the signs which I have enumerated, separately and collectively considered, we may in general be able to form some prognostic of the nature and event of the fevers of Jamaica. If to these we add those indications, which may be taken from the state of the eye and countenance, we may attain a still more satisfactory knowledge. It is an observation which I have constantly found to be true, that where the eye and countenance were serene and cheerful, the disease was void of any latent malignity, though it might be otherwise of a dangerous nature. On the contrary, where the appearance of the eye was sad, watery, inflamed, or uncommonly glistening ; where the countenance was of a dreary hue, downcast, dark and clouded ; and sometimes where it was of a beautiful blooming colour, which was not natural to the patient, there was always reason to suspect danger. But though a serene and cheerful eye and countenance are generally indications of safety, I must not at the same time omit to mention, that it sometimes happens in beginning mortifications, or in imperfect or unfavourable crisis, that the eye and countenance assume, for a short time, this flattering appearance of serenity and composure, though the hour of death is actually approaching fast. The indications from the eye and countenance are of the greatest importance, in enabling us to form a judgment of the event of fevers ; but little of this knowledge is, in fact, communicable in words. It must be drawn, in a great measure, from our own observations.

I do not pretend that the signs of prognostic, which I have enumerated in the preceding pages, are by any means complete, if referred to fevers in general; but I at the same time believe, that they are less defective, if applied more directly to the remitting fever of Jamaica. They were collected at a time when I was not much acquainted with books: and, on that account, I am induced to offer them to the public with more confidence; particularly, as I find that the indications, to which I have principally trusted, appeared in the same light to some authors, who are universally considered as careful observers, and who practised in climates, in many respects, similar to that of the West Indies. I have discriminated, as far as was in my power, between the doubtful and more certain appearances of danger or safety; and I hope I have no where advanced any thing, which has a tendency to mislead the uninformed. I may add, that general knowledge in prognostic goes no farther than a very rude outline, which individuals must fill up from their own experience. There are, in fact, few signs in fevers, which are absolutely decisive in themselves; and as these signs are often variously combined, so they must be separately and collectively estimated. It is only from considering accurately the result of the whole, that we can be enabled to speak with confidence.

C H A P. VIII.

OF THE DIFFERENCE BETWEEN A CRISIS AND
SIMPLE REMISSION.

IT is certainly a matter of some importance, to be able to distinguish between actual crisis and the simple remission of the fever of Jamaica; but it is a matter about which the practitioners of that country did not seem much to concern themselves. A difference undoubtedly exists, and the marks of it appeared to me clear and unequivocal in most instances. It was otherwise in the intermitting fever of America. In the pure intermitting fever of that country, I sometimes guessed luckily; but I cannot say positively, that I ever discovered signs on which I could depend with absolute certainty, that the fever was gone, not to return again, till the hour of return was past. In this disease, on the contrary, I should not expect to be deceived once in a hundred times. Much of this information, however, is too minute to be intelligibly explained in words, and therefore can only be acquired by actual observation.

I shall attempt to enumerate the principal of those signs, from which we may be enabled to attain some knowledge of the difference between actual crisis and temporary remission: and in the first place I remark, that the tongue was usually rough and dry, even during the most perfect remissions of the endemic fever of Jamaica. If it therefore happened, that it assumed a smooth and moist appearance at the end of a paroxysm, there generally was reason to believe that the fever was gone, not to return again. This presumption was still stronger, where its edges acquired the cherry-like colour of health; and particularly where the coat, with which it was usually covered, shewed

a disposition to loosen and separate. It must however be remarked, that though these appearances of the tongue afforded a common mark of the termination of fevers, they did not by any means afford a decisive one. Instead of crisis, they sometimes only indicated a change in the mode of action of the febrile cause; they were, in short, in some cases only forerunners of symptoms of nervous affection, or marks of a change from a continued to a remitting or intermitting form. Such are the presumptions of actual crisis, which might be drawn from the appearances of the tongue, where the tongue happened to be remarkably changed from its natural appearance in the preceding course of the disease. But it also sometimes happened in cases of the single tertian, where the paroxysms were slight, and the remissions long and perfect, that the tongue was so little altered by the presence of the fever, as not to afford any certain criterion between the remission and actual crisis. In some fevers likewise of a malignant kind, the tongue was sometimes smooth and moist, even red and clear on the edges, whilst the disease was advancing rapidly. This, however, so far as I have seen was constantly connected with a particular state of the stomach, viz. with nausea, or with vomiting of a viscid liquor.

Signs of crisis taken from the pulse alone, were not in general much to be depended upon in the fevers of Jamaica; yet, together with other circumstances, the state of the pulse might often help us to decide in doubtful situations. Changes from bad to better, if no symptoms of comatose affection appeared at the same time, were generally considered as indicating crisis, or tendency to crisis; yet it will be less expected, perhaps, that I should rank the intermitting pulse among the signs, which indicate a favourable termination of the disease. Some instances of this have occurred to me, both in the West-Indies and in England; but though I mention the fact, I confess myself unable to offer an explanation of it.

The state of the skin affords marks more decisive of the total or temporary absence of fever, than the ordinary state of the pulse. When the sweat, which in the preceding remissions had been partial and imperfect, became copious, fluid, universal and of long continuance, there was generally a presumption of crisis. But independent of the nature of the sweat, there is something in the state of the skin, something in the impression which it makes on the hand which feels it, very different when the fever has only remitted, and when it has terminated finally. Though it may be difficult, perhaps impossible, to mark this distinction precisely in words; yet it is easily known to those, who accustom themselves to observe minute circumstances with attention. There was, in fact, no indication of that spasmodic stricture on the surface, which had been observable in the former remissions, and we might say, that the body was perspirable, even in the extreme parts. It is a circumstance likewise not a little curious, that the heat of the body, during a crisis, particularly in those fevers, which had principally affected the nervous system, was sometimes so much below what it usually is in health, as to be really alarming. In some instances, I have found the extremities to be not less cold, than if the patient had been actually dead; yet this coldness was of such a nature, as to be distinguished without much difficulty from that which precedes death.

The above is only a very imperfect history of those signs, which actually distinguish the crisis of fever from a simple remission. There are still some others, not less to be depended upon, perhaps, but which cannot be so easily reduced to distinct description.— Among these, we may reckon unusual evacuations upwards or downwards, sound and refreshing sleep, where watchfulness had prevailed through the preceding course of the disease, return of natural appetites, decrease of thirst, loosening of scabby eruptions, and

above all, a certain expression of chearfulness in the eye and countenance, which though not to be defined in words, conveys to the mind of the observer, a strong conviction of what is going forward. This brightness of the eye was well known to Hippocrates, as a salutary sign in fevers; but though it generally affords a very decided indication of a favourable event; yet we must be careful to distinguish from it that clear and glassy appearance, which the eye sometimes acquires previous to death.

C H A P. IX.

THE GENERAL CURE OF FEVER.

BEFORE I attempt to offer rules for the treatment of the remitting fever of Jamaica, it will be proper to consider in the first place, how far the cure of the disease is the work of nature, and how far it already has been, or hereafter may be accomplished by the exertions of art. The question is important, and till its limits are defined, we cannot hope to establish rules of practice on a firm basis, or to conduct a mode of treatment on a consistent plan. It will be necessary however, before entering farther into this subject, to consider the power of the *vis medicatrix naturæ*:—a principle, which under one name or other, has influenced the views of medical men from the earliest records of physic, till the present times.

SECTION I.

OF THE *VIS MEDICATRIX NATURÆ*.

IT is an opinion, which seems either to have been expressly avowed, or tacitly acknowledged in every age of the world, that a fever is a combat or effort of nature, to remove from the system the derangements of a morbid cause; or in other words, to restore a diseased body to its ordinary health. It was long believed, that the powers of the constitution made an attempt to concoct the crude and undigested humours;—and finally to expel them from the body. But this mode of reasoning is found to be unsatisfac-

tory: and some late authors have adopted the idea, that nature directs her efforts towards effecting a solution of spasm, on the presence and obstinacy of which the disease has been thought, in some measure, to depend. It will be a hard task to reconcile this difference of opinion, or to say which is the right one. There are plausible arguments on both sides of the question; and neither supposition, perhaps, is accurately true. The coction of humours (as it may be called) is often apparently connected with evident approaches towards a relaxation of spasmodic stricture; and increased discharges by the different excretories, are obviously attendants of its actual solution. Yet though this is certainly true, it still is not clear, that either the coction of the humours, or the solution of the spasm, is the real effect of a regular mechanic operation of the powers of nature, attempting by this means to overcome the destructive tendency of the disease. It is even more probable, that the coction of the humours, or solution of the spasm are only circumstances of accident, occasionally connected with certain states of action of the morbid cause; but which do not arise from the regular design of nature to accomplish this purpose. I just now observed, that there is a difference of opinion about the mode of operation, which nature employs to combat the effects of the disease; but I may add, that no body, as far as I know, attempts to deny the existence of such a principle in the constitution of the frame, as disposes it to restore its own health by a certain train of regular efforts. On the first view of the subject, indeed, there are many circumstances which give countenance to the opinion. The relief which often follows hemorrhage, sweat and other evacuations in unusual quantity; and still more, the relief, which attends the eruption of the small pox, or the appearance of gout on the extremities, undoubtedly affords a strong presumptive argument, that nature raises

some active and generally well directed efforts, which remove from the body a cause that disturbs the ordinary functions of health. But though the above circumstances are commonly known, and may be supposed to afford an argument in support of this opinion; yet the fact may perhaps admit of another explanation, while there are other appearances, connected with the subject, which render the existence of this regular design of nature very questionable. When I say, that I cannot readily allow the *vis medicatrix naturæ*, (in the sense in which it is generally understood), to be an established principle in the constitution of the frame, I am aware, that I incur an imputation of ~~of~~ presumption. An opinion, venerable from antiquity, and supported by many plausible arguments, might be thought to be secure from the attacks of a man, who has no professional reputation to boast of: yet as no desire of novelty has induced me to fabricate a conjecture on a dark subject, so I humbly hope I may be indulged in my attempt to explain an important truth. The sanction of two thousand years, and the authority of the names who support this doctrine, are formidable opponents; yet I trust I may be able to prove, that the *vis medicatrix naturæ* does not, as is commonly believed, restore the health of the body by one general and uniform mode of operation; or that our diseases are not removed in consequence of a regular design in the mechanism of the frame. I shall relate the fact from which I have been led to form this conclusion; for the refutation or confirmation of which, I require no other indulgence than a candid enquiry.

I have just now declared, that fever, or the cause of fever, is not combated and finally overcome by a regular train of active efforts, or a *vis medicatrix naturæ*: and I must observe in proof of it, that there were many of the fevers of the West-Indies, where the disease, or the paroxysm of the disease, vanished or

declined, without any exertion on the part of nature being perceivable. The powers of life, during this period, were in some degree suspended. The patient, who could only be said not to be actually dead, was totally insensible to every object that was near to him; and often did not feel the irritation of acrid substances that were applied to him: yet after a certain continuance in this state, he began to resume his powers of sensation and motion; and the disease at last subsided or vanished, though the efforts of nature were not discovered; nay, though the vital powers were sometimes in a state so weakened, as to be apparently very little capable of effort. This fact, which the most superficial observer could not easily overlook, furnishes sufficient reason for doubting of the very existence of a *vis medicatrix naturæ*; a doubt which is further corroborated, by observing the manner in which death frequently approaches in the fevers of Jamaica. It is known, that the sufferings of the patient are sometimes alleviated for a short time before death. This alleviation, wherever it does take place, happens constantly at the expected period of crisis. The cause of it has generally been attributed to the *vis medicatrix naturæ*; that is, to a last effort of the powers of life; but I have weighed carefully all the circumstances connected with the phenomenon, and cannot readily assent to the opinion. To indulge in conjectures, is contrary to the principles I profess; yet I must suggest, that a fever, or the paroxysm of a fever, terminates, strictly speaking, from a hidden something in the nature of the febrile cause, from something which ceases to act, or which changes its mode of action after a certain duration. I do not pretend to explain the manner in which this happens. I only add, that the fact is supported by probabilities. It was frequently observed, in such cases of fever as terminated fatally, that there was actually a period of time, generally the period immediately preceding a

decided fatal termination, where the real presence of disease was perceived with difficulty. The patient, however, was unable to recover. Death happened in a very few hours, and it seemed to ensue in such cases, from one of the following causes, viz. either from the mechanism of a part of vital importance being destroyed; from the powers of nature being too much exhausted to continue life; or perhaps still oftener, from the recurrence of the disease, in another form, speedily putting a period to existence, while the vital principle was in this weakened state. But though the circumstances, which I have mentioned, afford grounds for believing that a fever, or the paroxysm of a fever, is not actually removed from the body, solely by the efforts of a *vis medicatrix naturæ*; yet if we choose to proceed further in the investigation, it will be no difficult task to involve this opinion, which has hitherto been considered almost as sacred, in still greater perplexity. If we admit of the existence of a *vis medicatrix naturæ*, it will not be easy to conceive, how a fever, which has once been expelled from the body, should return again in a given time, or how the alternate paroxysms of the double tertian, for instance, should be of such different duration or of such different degrees of violence in the same person; neither can we understand, how a fever of one kind should last only seven days, another fourteen, and another twenty, or longer:—circumstances which happen daily, without the least apparent connexion with the innate vigour of constitution. We shall be equally puzzled likewise, if we attempt to explain on the supposition of this principle, how a fever should continue, while the powers of the constitution are vigorous and strong, and cease when they are apparently exhausted. The above are well known facts, and do not leave any room to doubt, that the termination of fever, or of the paroxysm of fever, depends on some other principle besides the mere efforts of the *vis medicatrix*

naturæ. Whether this resides, as was hinted before, in a hidden modification of the nature of the cause, which ceases to act, or changes its mode of action after a certain duration; or whether, combined with this, the constant but imperceptible changes, which are continually going on in the system, destroy in the frame of the individual, that particular state of aptitude to the febrile cause, in which the disease consists, we cannot determine with certainty; yet it would be obstinate to maintain any longer, that the cure of fever is owing to general and well directed efforts of nature, expelling a morbid matter or overcoming a prevailing spasm. It is true, that an obvious solution of spasmodic stricture, or the appearance of a morbid matter on the less important parts of the body, usually attend the favourable termination of fevers; yet these circumstances are, in fact, attendants rather than causes of crisis. I do not deny, that increased discharges by different outlets, sometimes moderate the violence of fever during its continuance, as well as attend its final solution; yet it has not, nor perhaps can it ever be demonstrated, that this proceeds from a regular design of nature.

The hints which have been thrown out in the preceding pages, give room for supposing that the *vismedicatrix naturæ*, in the sense in which it is usually understood by medical writers, is only a principle of doubtful existence in the constitution of the frame; yet though this is certainly true, I do not pretend to deny, that the animal machine is endued with a power, which resists, in some measure, the derangements of a destroying cause, and which persists to a certain degree in continuing the action of living. The general nature of the cause of fever, or the nature of its various modifications is a mystery, which we do not as yet know. We only know, that when present in a certain state of vigour and activity, it deranges or disturbs the actions and functions of the system;—

while we likewise know, that it does not always disturb every action or every function in the same degree. It has occurred too often to have escaped the most superficial observation, that where one part of the body suffers particularly, the others are often relieved in proportion. We frequently in this manner observe, that general fever is diminished by the appearance of local pain; or, on the contrary, increased by its removal. It likewise often happens from the same principle, that where the stomach and biliary system suffer much, there is less disorder in the other parts: and on the other hand, that where these sufferings are removed or mitigated, the general fever runs higher, and often continues high, till the same, or other local affections, are again produced. Thus, though we are totally ignorant of the intimate nature of the cause of fever, we still perceive very plainly, that it either possesses something in its own nature, or accidentally meets with something in the constitution of the individual, which determines it to affect the different parts of the body in an unequal degree. It usually exerts its greatest force upon parts, which are preternaturally weakened by the general influence of climate, season, situation, or other accidental causes. Hence bilious appearances are common in the hot months of hot climates, pneumonic affection in cold and dry weather, greater degrees of vascular excitement among the temperate and more active races of men; while symptoms of nervous affection prevail among the luxurious and enfeebled. The above, with other species of the increased action of the cause of fever on a particular part of the body, depend wholly, perhaps, on circumstances of accident; yet it has so happened, that those irregular determinations have unfortunately been considered as the efforts, which nature employs to expel from the body a cause, which disturbs the economy of health. I shall not, at present, go so far as to contend, that

these determinations are not, in fact, intentions of nature; but shall only beg leave to suggest, if they actually are intentions, that it is mere chance which determines whether they are salutary or fatal. It is a truth which nobody will deny, where the force of the disease is accidentally directed to an organ of excretion, or to a part of little importance to life, that the rest of the body is often proportionally relieved, and even that a recovery of general health is sometimes the consequence; yet the contrary is the effect, where the functions of the part, upon which the force of the fever has been thus accidentally diverted, are of immediate importance to the action of living. The Gout, a disease, the cause of which bears no very remote analogy to the case of fever, may be adduced as affording an illustration of this truth. The proximate cause of gout, is equally hid from us as the proximate cause of fever. We know, however, that the one equally with the other, has a tendency to destroy life. We likewise know, that there is a power or principle in the constitution, which to a certain degree resists destruction. The nature of this power, however, is unknown. We are not only in the dark with regard to its nature; but we can only form conjectures about the part where it principally resides.—We, however, clearly perceive its force and activity to be different in different parts of the body. We may next be allowed to remark, that where the cause of gout is in a certain state of modification, tumults, (which properly enough may be termed re-action), arise in the system, and go on to continue till this cause or hurtful matter finds an outlet from the body, or a lodgment on one particular part. The outlets from the body are numerous: the parts on which the gout seems principally to fix its seat, are the extremities, where the power of resistance is smallest.—The vital principle, however, becomes weaker as man advances in years; and the cause of the disorder

seems then frequently to find accommodations in parts, which are less remote from the sources of life. This more especially is the case, where tone and vigour have been preternaturally weakened. Hence the stomach, the bowels, sometimes the brain, and even the heart itself suffer from the immediate action of this disease, in the latter periods of life. But though no person perhaps will deny, that the cause of gout finds readiest accommodation, (if I may so apply the term) in those parts of the body, where the vital powers are naturally weak, or have been accidentally weakened from various causes; yet we may add, that it is likewise removed from the parts, on which it has been thus fixed, by such applications as excite their active powers; or, in other words, which call forth the local re-action of the system. We may also observe, that tumults arise in the general system, in consequence of this repression or repulsion of the morbid cause from a particular part; and that they do not in general cease, till an outlet is opened, or accommodation found in some other parts of the body. The above appearances, occur daily in the history of gout. They seem to bear a strong analogy to those irregular determinations, which frequently take place in fevers, and their cause perhaps is the same. We do not perceive any other law by which they can be explained, than the natural or adventitious state of activity of the powers of life, which resist destruction with unequal force in the different parts of the system: so that we shall be obliged to conclude, that those sufferings, which have hitherto been styled the efforts of nature, are in reality more of the passive, than of the active kind.

The circumstances which I have now mentioned, combat the very existence of the opinion, which has been commonly received with regard to the *vis medicatrix naturæ*. I have hinted, that the extent and limits of that principle are narrow, and that the sa-

lutory effects are accidental. I shall next endeavour to shew, that they cannot, without danger, be made the basis of the general plan of cure in febrile diseases. The task is important, but the attempt may be thought presumptuous, as an opinion, contrary to that which I advance, has obtained almost the universal consent of mankind. I have no desire of changing names, or of making distinctions, where there is in fact no difference. I perfectly acquiesce in retaining the word *vis medicatrix naturæ*, provided it is limited to a certain mode of re-action, or to a power in the constitution of resisting destruction unequally in its different parts, in consequence of which, irregular determinations sometimes prove salutary by accident; yet I must add, that if we mean to denote by this term a system of laws, which have the best directed tendency to remove from the body a cause which destroys health, and endangers life, the opinion has a very uncertain foundation. There are few persons so ignorant, or so blindly devoted to the doctrines of *αὐτοπατεία* as not to own, that the usually reputed efforts of nature, are often ill directed, sometimes pernicious: in short, that they are obviously the causes of death. The truth of this observation cannot be denied, and unfortunately it obliges the advocates of the *vis medicatrix naturæ*, to grant the conclusion, that the laws of the principle are imperfect. The works of the author of nature, as far as our limited knowledge can trace them, are universally without defect, if examined according to the plan on which they have been originally formed. If they appear otherwise, it becomes us to hesitate, before we decide. We may not have comprehended the fundamental principle of the design; but we revolt from the idea, that the execution would be left imperfect, had it been intended by the Author of our being, that the mechanism of the frame would be such, as should oppose and remove, in the most

effectual manner, the derangements of the morbid cause. Defect and imperfection can have no place in the works of the Almighty. Had it actually been in the original design of our Creator, that the human body should be provided with a system of the best concerted laws for restoring its health, when deranged by the numerous causes of diseases, as it is impious to suppose, that those laws could be defective; so we may reasonably conclude, that the effects of fevers would not then have been fatal. We find however, that fevers, as well as other diseases, are fatal to people of all ages and descriptions: and that nature's intentions of cure, if they really are intentions, are often destructive to herself. I need scarcely remind the reader of examples of their pernicious tendency. Vomitting, sweating, increased discharges by stool, &c. are generally considered as the salutary efforts of nature: but instances are numerous, where the excess of those evacuations have obviously proved the causes of death. In the same manner, abscesses, which in the remote parts of the body, sometimes attend, and even sometimes perhaps influence the favourable termination of fevers; in the brain, or in other organs of importance, are no less certainly the cause which destroys life. In both instances the design of nature, if it can be called a design, is the same. The force of the disease being turned principally upon one part, the rest of the body is in a great measure relieved from its sufferings; - but the health and structure of the part are hurt or destroyed by the change; and it depends wholly upon the accidental importance of the organ, upon which this diversion has been made, whether death or recovery is the consequence. Thus it often happens, that the reputed indications of nature prove the immediate causes which destroy the existence of the individual; a fact not reconcileable, with the infinite power and wisdom of the Author of our being.

I have insinuated, that the efforts of nature are uncertain and precarious. They depend on accidental determinations to different parts of the body; and I may add, that if we endeavour to investigate the cause, which directs the mechanism of the frame, to adopt one species of effort, or one mode of determination in preference to another, we shall not perhaps be able to find any other, than a difference in the states of the powers of life, which resist destruction with unequal degrees of force in the different parts of the body. Where there is the least resistance, either from the natural or accidental circumstances of the constitution, there the disease most obviously exerts its greatest force. Hence we are sufficiently warranted to conclude, that though the structure of the human body is perfect with respect to every purpose for which it is intended, being only endowed with a principle, which resists destruction, or persists in continuing life to a certain degree; yet that it is extremely defective, if we consider it as a machine furnished with a system of laws, which have an invariable and well directed tendency to restore health by the most judicious and rational efforts. The restoration of health, in consequence of this re-action, or irregular determination which takes place in the system, is only a circumstance of accident. The skill of man sometimes succeeds, where the efforts of nature have obviously failed.

SECTION II.

OF THE GENERAL INDICATIONS OF CURE IN FEVERS.

THE vis medicatrix naturæ, has been hitherto esteemed a principle of much importance in the cure of febrile diseases. I have attempted to ex-

plain its real limits and extent ; but am afraid I may not have done it satisfactorily. A tumult which, properly enough perhaps, may be termed a reaction of the system, evidently takes place in consequence of the application of a morbid cause ; but there seems to be little reason for believing, that this reaction points out the best method of cure, or wholly by itself accomplishes this important business. But though the reputed efforts of nature are thus defective in accomplishing the cure of fevers ; yet I do not deny, that there is a general tendency in fevers, or in the paroxysm of fevers, to terminate in a given time, often by a fixed and regular mode of termination. We do not, however, by any means comprehend the cause upon which this depends. From the similarity in the progress and termination of epidemics, as well as from the steadiness with which various sorts of fevers pursue their course, in spite of the most opposite modes of treatment, we are led to conclude, that there is something peculiar in the modification of the cause, which influences the duration of the disease. This at present, is unknown ; perhaps is a knowledge which we cannot attain ; yet if we take pains to observe the course of fevers with attention, we may discover some rules of practical use. We know that one species of febrile diseases, obstinately pursues its course, notwithstanding every endeavour to oppose it ; while another is so totally under our management, as to be stopt short at pleasure with almost infallible certainty. It thus happens, that the intermittent is perfectly under our controul. Over the continued, and even over the remitting fever of Jamaica, I am afraid, we shall be obliged to confess, that we possess no very certain power.—But I shall examine this subject more particularly.

When I first arrived in Jamaica, in the year 1774, I found that the practitioners of that country very generally believed, that the course of the ordi-

nary endemic fever was checked with great certainty by the powers of Peruvian bark. This opinion, indeed, is found in every medical book, and it appeared frequently, on the first view of the subject, to be well founded. No great space of time, however, elapsed before some circumstances were observed, which presented the matter in a different light. I found in many instances, that bark was given in the first remission, or on the second day of the disease; in others, it was not given till the third remission, or till the sixth day from the beginning of the complaint; and in some again, the fever disappeared altogether before a single grain of this remedy had been administered. I was particularly exact in marking the time or the period of the disease, at which the bark was begun to be given, as also the quantity which was taken upon the whole. The result was not such as might have been expected. Notwithstanding the most opposite modes of treatment, the disease appeared to terminate or change about the same periods in almost every patient. This fact was confirmed in numerous instances; and it seems to afford a very unequivocal proof, that bark, in the quantity in which it is commonly prescribed in the West Indies, has not the effects which are *usually* ascribed to it. I do not, however, infer, that this remedy may not be capable, with more decisive modes of management, of effecting all that has been expected from it. I had not, during the time that I remained in Jamaica, any conception that the stomach could have retained, or that it would have been safe to have ventured upon the quantities of bark, which I afterwards gave to others, or took myself in the intermitting fever of America. Two scruples or a drachm, every two hours, is in fact only a small dose. To this under dose, during short remissions, we might perhaps impute the failure of that remedy in the fevers of the West Indies. That this actually is the case, is confirmed

in some degree, by an instance which I find recorded among my notes. A young man was seized with a fever, about the middle of August, which shewed marks of great violence from the beginning. Bark was given early, and in larger quantity than customary. The last paroxysm of the disease, was in some measure suspended, in consequence of this proceeding; yet, except that the marks of external fever were obscure, the patient remained, as usual, uneasy and distressed, till the period at which the crisis was expected; when the marks of final termination shewed themselves distinctly. This is the only case I met with, where the paroxysms of the fever of Jamaica were stopt, or suspended by the bark; or where external marks of fever vanished without evident signs of crisis. It affords only a doubtful proof of the power, which this remedy has been supposed to possess, of absolutely cutting short the course of the endemic of that country. But though the bark was seldom efficacious in abruptly cutting short the course of this disease, it is no more than justice to remark, that it is a remedy which was almost every where safe, and that it was ultimately useful in promoting the cure. It imparted in most instances, where it was employed, a degree of tone and vigour to the system—a certain something to the constitution; in consequence of which, the crisis, which we should have expected to be only partial or imperfect, became decided and final. I have suggested those few remarks, with regard to the virtues of Peruvian bark in the common endemic of Jamaica; if its effects are so very doubtful in this disease, we have no reason to expect, that they will be more certain in fevers of a more continued kind.

Besides bark, the power of which appears to be very precarious, other remedies have been employed by physicians, with the view of cutting short the course of fevers.—Antimony, under one form or

other, has been celebrated for this intention, since its first introduction into medicine. James's Powder is the most famous, and perhaps the most effectual antimonial preparation, which, as yet, has been offered to the public. I am sorry to say, that I had not an opportunity of making proper trial of it, in the fevers of the West-Indies ; but I can add, that the emetic tartar was often found to be dangerous, scarcely ever effectual in cutting short the course of the disease, unless given at an early period, or before the fever had assumed a proper form. Its *virtues*, as a febrifuge, were heightened by the addition of opium and camphire, I am sorry also to remark, that I cannot speak with confidence of the virtues of James's Powder, in the intermitting fever of America. Emetic tartar was frequently employed, but it did not by any means answer the expectations which were entertained of it. I acknowledge, that it might be so managed, as apparently to prevent the return of a particular paroxysm ; but the instances, where it completely removed the disease, were so rare, that I do not consider it as possessed of very eminent virtues. I have had frequent opportunities, since my return to Britain, of trying James's Powder in the continued fevers of this country ; and the result of my experience inclines me to believe, that this remedy, when given at an early period, has sometimes actually carried off the disease. It appeared likewise, when exhibited near the critical periods, to render the crisis more complete ; but I have little cause for thinking, that it ever cut short a fever in the midst of its course. Thus it appears, that these two celebrated remedies,—bark and the various preparations of antimony, are, in fact, less effectual in cutting short the course of febrile diseases, than has been commonly supposed ; I cannot, however, abandon the idea that the purpose, which has been expected from these remedies, may still be obtained by other means.

Galen mentions some instances, where he extinguished the fever by copious evacuations : authors mention many, and I have myself seen some, where drinking plentifully of the coldest water, produced the same effects. The alternate use of warm and cold bathing occasions great changes in the state of the constitution : and from the trials, which I have made of these applications, I do not entertain a doubt, that they may be so managed, as to shorten very materially the duration of fevers. I do not however promise, that they are capable of being so conducted, as infallibly at once to stop the disease in its progress. This can only be accomplished by those great and remarkable changes, which destroy a certain aptitude, in the state of the system, to the morbid cause, in which the disease is supposed to consist. But I must at the same time confess, that as we neither know the nature of this aptitude, nor the particular nature of remote causes, so every attempt of cure on this plan, as it must be at random, cannot be adopted without danger. It is a view, therefore, which will not be prosecuted with safety, while our knowledge of the nature of morbid causes, and of the laws and structure of the human frame, is so imperfect.

It is evident from the facts which I have related in the preceding pages, that we cannot safely trust the cure of fevers to those tumults, or irregular determinations in the system, which are usually styled the efforts of nature : neither does it appear, that we can depend on the efficacy of any one remedy, we are yet acquainted with, as possessed of the power of abruptly cutting short their course. We still however perceive, that these diseases have a general tendency to terminate in a given time and steadily to go through a regular progress, in spite of the greatest exertions of art. If we review the practice which medical people have followed in fevers, from the days of Hippocrates to the present times, we meet with such contradictory methods of treatment, as render

it impossible to avoid pronouncing, that if one man had actually saved life, another's endeavours seemed as if intended to destroy it: yet few authors have ventured to offer the fruits of their labours to the public, without previously boasting more successful methods of cure, than were known to their predecessors. Hence, if we are not sometimes disposed to doubt their veracity, we can hardly avoid concluding, that their practice had been feeble, and of small effect. We lament, with reason, that medical facts are frequently of little value: nay, that they oftener mislead, than guide us in the way to truth. An overfondness for ourselves is, perhaps, more the cause of this, than real want of candour; the natural propensity of the human mind to flatter itself, disposing us to attribute cures to remedies, which were administered near the critical periods of the disease; while twenty instances, where similar treatment produced no apparent effect, are insensibly blotted from the memory. This at least was the case with myself. I flattered myself in many instances, that I had actually saved life:—I now find, on maturer reflection, that I had in reality done no material good. Thus it frequently happens, I believe, that practitioners boast of cures, to which they have no right; at the same time I am convinced, that they are frequently charged with deaths, of which they are innocent. The life of man does not appear to depend upon so small a matter, in febrile diseases, as is generally imagined; and is not often preserved or endangered by the routine of common practice. It is not always easy to know exactly the real effects of treatment; neither has the road, which leads to this knowledge, been pursued with sufficient industry. Fevers occur frequently, and on that very account, have been traced less minutely in their course, than some other diseases. There are few practitioners, who write down in the presence of the sick, a minute and accurate history of the various cases of fevers, which come under their care; who observe

carefully the changes which happen from day to day; who note the particular methods of cure, and the effects which arise in consequence of every alteration of treatment. Yet, unless all these circumstances are attentively considered, we cannot hope to form conclusions, which are in any degree to be depended upon. If we defer making remarks till the patient recovers or dies, difficulties will be easily got over, and such facts as contradict opinions, in which we have long believed, will be more easily reconciled, as being less perfectly remembered. Hence it is that a man may continue a very extensive practice, for a very long life, without ever once getting a view of the real truth.

As from what has been said before, little doubt can remain of the precariousness of trusting the cure of fevers to the simple efforts of nature; or, if we except intermittents, to the efficacy of particular remedies which cut off their course abruptly, it remains to look out for some other principle, which may serve us as a guide in the conduct of our practice. It is a truth to which we may accede without hesitation, that the cause of fever, as I have mentioned before, whatever it is, or however modified, has a general tendency to destroy the powers of life; while we likewise know, that there is a principle in the mechanism of the frame, which resists destruction to a certain degree. We also know, that the cause of the disease differs in degree of force; and that it is differently modified according to various circumstances; as we likewise observe, that the principle of life, or power of resistance, is different in activity, in the different parts of the body. To these two powers, viz. the force of the morbid cause and the powers of the constitution our views in practice must be principally directed. Hence we may establish a general rule, that wherever the force of the morbid cause is weak; at the same time that the powers of life in the general system, and particularly in the organs of vital importance, are

strong and active, we have nothing more to do than to look on. On the contrary, where the cause of the disease is of unusual force, or where the powers of life are preternaturally weak, our interference ought to be speedy, bold and decided. In other words, we leave the business chiefly to nature, or take it entirely out of her hands. It is this which a practitioner must first determine, when called upon to give his assistance to a patient labouring under a fever. If the powers of the constitution appear to be equal to the task, our interference would be officious, and perhaps might prove hurtful. If assistance is necessary, it ought to be our principal aim to render it complete; but in prosecuting this view, we meet with much difficulty and impediment. We are not yet acquainted with any one remedy, which has a certain and infallible power of cutting short fevers in the midst of their course. It is not therefore absolutely in our power to take the business entirely out of the hands of nature. We can, in fact, go no farther, than to oppose her pernicious efforts; or to obviate the fatal tendencies of the disease. The fatal tendencies of the disease, are variously modified, and the means by which they must be obviated, are sometimes directly opposite. Two general modes, however, of the fatal action of fevers may in most instances be discovered. The cause of the disease, in one case, exerts its influence on the sources of life and motion; in the other, the structure of an organ of less importance is destroyed, and death happens only from a secondary effect. There is perhaps no fundamental difference in these different modes of action; yet the indications of cure, which arise from this view, are totally opposite. In the first instance, it is necessary to excite, and to support the general powers of life: an indication of very great extent. In the other, it is sometimes necessary to diminish the general reaction of the system; to obviate irregular determination, and to oppose with vigour the tumultuary efforts of nature.

C H A P. X.

OF THE PARTICULAR CURE OF THE FEVER OF
JAMAICA.

I SHALL begin this subject with observing, that the fever, which prevailed in the district of Savanna la Mar, was naturally a disease of the remitting kind; yet circumstances were sometimes connected with it, in such a manner as prevented it from assuming its proper form. To remove those circumstances, which thus masked or concealed the real genius of the disease, was considered as the first step towards a cure. The accomplishment of this purpose, however, was sometimes difficult; neither could it always be effected by the same means. Thus it happened frequently in cases, where there was excess of excitement, or a high degree of inflammatory diathesis, that the remissions were scarcely perceptible; as it was likewise observed, that where there was a want of reaction, the paroxysms were often languid and obscure. In the one case, the remissions discovered themselves in consequence of bleeding, dilution and copious evacuation; in the other, wine and cordials determined the disease to assume its proper genuine form.

In the first place, evacuations were usually employed as the means of procuring remission, where the inflammatory diathesis prevailed in excess; I may add, that they were proper for the most part, and that they seldom failed of producing the effect. Bleeding was frequently necessary, and generally of service. Its efficacy, however, was often heightened by particular modes of management. Thus relaxation of spasms, and

removal of inflammatory diathesis, more certainly followed bleeding, if the blood was drawn from a large orifice; if the patient was placed in an erect posture, during the operation; and more certainly still, if the lower extremities were at the same time immersed in warm water. When bleeding had been premised, and repeated according to the circumstances and urgency of the case, it was then customary to open the body freely: for which purpose, I have not found any thing answer better, than a thin solution of Glauber or Epsom Salts, with a small portion of emetic tartar. The operation of this medicine was extensive. It might be so managed as to occasion nausea, or moderate vomiting; to operate briskly downwards, or to promote a gentle diaphoresis. Remissions were generally the consequence of this method of proceeding, where there was no defect in the manner of conducting it. But where it so happened, that the circumstances of the patient forbade the use of this laxative; or where it might not be proper to carry it to a sufficient length, benefit was derived from a powder, composed of nitre, camphire, emetic tartar and opium, given in pretty large doses, and repeated frequently. Remission, at least a great abatement in the violence of symptoms, was generally the consequence of this plan of treatment; particularly, if assisted by the plentiful dilution of watery liquors, by warm bathing and by large glysters of simple water. It is superfluous to mention the use of blisters in cases of local affection; but it will be less expected, that this remedy should be recommended in fevers, where there is an excess of the general inflammatory diathesis.—I can, however, bear testimony to its efficacy. The manner by which blisters produce their effects, is not yet agreed upon among authors; neither do I pretend to throw any new light upon the subject; but I would beg leave to suggest, that the mode of affording relief in the present, at least, did not seem to be much

unlike the effect of local affections, in consequence of which the violence of fevers is sometimes observed to subside.

I pursued the above method of procuring remission in those fevers, where there was real inflammatory diathesis prevailing in excess; but it so happened, that the signs of this diathesis were fallacious, appearing in some instances to be present, though the real genius of the disease was actually of a different nature; a circumstance, which occasioned a difference of management in conducting the method of cure. Excessive evacuations were not only unsafe in such cases, but in general had not any powerful effects in disposing the disease to assume a remitting form. Bleeding, however, was often found to be necessary, though it was seldom requisite to repeat the operation. The good effects which were observed to follow the use of cathartics, were not in general very remarkable; yet it was proper, in most instances, to open the body freely; for which purpose, no form of medicine, with which I am acquainted, answered better than a solution of salts with a small portion of emetic tartar, and sometimes with the addition of laudanum. In cases of local pain, blisters applied near the seat of affection were always of eminent service; and in cases of general irritability, they were often equally useful, when applied to the back part of the head and neck. A powder composed of nitre, camphire, emetic tartar and opium, was likewise employed with success; but the liberal use of warm bathing, was still more to be depended upon. No person, perhaps, will refuse consent to the method of proceeding, which I have hitherto recommended; but when I mention a free and bold use of cold bathing, even in an early stage of this fever, I do not expect the same concession. To dash cold water on the head and shoulders of a person in a fever, has an appearance of rashness and hazard. I can, however,

produce the testimony of repeated experience for the safety of the practice, no less than for its success in procuring remission; and shall therefore consider it a duty to recommend it warmly to the public. Wherever it was employed,—and the cases in which it was tried were numerous, a calm and equable perspiration, additional tone and vigour, with great abatement of irritability, were constantly observed to ensue.

The paroxysms and remissions were generally distinct in the beginning of fevers, where the nervous system was principally affected; but often became less so, as the disease advanced in its progress; a circumstance which did not arise oftener from the nature of the complaint, than from the common method of treatment. Bleeding was often dispensed with in the fevers of the West-Indies; but vomiting and purging were indulged in with freedom. The distinction of paroxysm and remission was sometimes evidently rendered obscure by this practice; while it was likewise obviously restored again, by the use of wine and cordials, which excited the powers of life. In this species of disease, evacuations were seldom necessary; seldom indeed admissible in a great extent. Bleeding unless under particular circumstances, was totally improper. Cathartics were sometimes dangerous, and antimonial vomits often sunk the patient irrecoverably. Blistering, on the contrary, even at an early period, was generally of service; as also were opiates, and a judicious use of the warm bath; but cold bathing with salt water, was, of all others, the remedy of the most powerful effect. I do not pretend to say, that it absolutely stopped the course of the fever; but I can say with truth, that it generally restored the distinction of paroxysm and remission, diminished irritability, and imparted a degree of tone and vigour to the system, which was justly considered as a sign of safety.

To procure remission in fever, distinguished by a prevalence of the putrescent tendency, is not in every

instance an easy task. A remitting fever, with marks of specific putrefaction, is not a disease of common occurrence in Jamaica; but a fever with signs of putrefactive tendency, mixed with symptoms of great irritability, or a high degree of malignity, is not altogether rare. From the complicated nature of the disorder, the indications of cure are often difficult and perplexed. Bleeding is universally condemned; more, I believe, from theory than from actual observation. It was, and perhaps still is, a fashionable mode of reasoning, to impute the languors and other marks of debility, which are common in the fevers of the West-Indies, to a putre cent tendency in the system. Such symptoms however are in fact more generally the attendants, or distinguishing signs of fevers, where the nervous system is affected. In such cases, bleeding is obviously hurtful; in the one of which we now treat, (where such a disease actually exists,) it is not only a remedy of safety, but of very eminent service, previous to the application of cold, particularly previous to cold bathing, which may be used with freedom and boldness. Cold bathing, indeed, is the remedy on which we must principally depend. There are others which do good occasionally; but this is the only one I know, which has any very considerable effect in changing the nature of the disease. There is a general rule in the practice of medicine, which requires to be particularly remembered in those complicated species of fever, viz. that as the indications of cure are often embarrassed; so the appearances, which principally point to danger, are first to be attended to; while the plan of cure, which we determine to be the most proper, must be followed up with vigour and resolution. We ought always to bear in mind, that in dangerous and difficult cases feeble remedies, or even powerful ones timidly used, are of little avail. Cold bathing, employed with timidity, failed of doing good in some instances. I

met with no example, where the boldest use of it did harm. It was seldom, I must again repeat, that it did not succeed in obviating irritability, in checking the putrescent tendency, and in imparting to the system that degree of tone and vigour, in which safety is observed to consist.

The method of procuring remission, in those fevers which were distinguished by local affections, or irregular determinations to particular parts, was nice, and sometimes difficult. Bleeding was frequently proper, especially, if there subsisted at the same time marks of a general inflammatory diathesis: but it was seldom sufficient wholly to accomplish the business. However, together with a judicious management of warm bathing, it greatly heightened the good effects of blisters, the remedy on which the principal dependence was placed. In fevers which were accompanied with uncommon pain of the head, I have sometimes found it serviceable to apply cold to the part affected; the feet being at the same time immersed in warm water, and blood flowing by a large orifice from the arm. I also frequently observed, that the general fever ran higher, though it likewise more certainly assumed its proper form, in consequence of bleeding, blistering, and the removal of local pain. In those fevers, where bilious appearances were the effect of accidental, irregular determination to the stomach or liver, the remissions were often obscure: neither did the method of treatment, which was generally adopted, seem to be well calculated to bring forth the natural, genuine appearance of the disease. Bilious appearances, it must be owned, sometimes vanished, while the type of the fever became more distinct after the exhibition of an emetic or brisk cathartic; yet there is cause to doubt if this depended on the evacuation of bile. It might be said, with more truth, perhaps, that the action of the emetic, by exciting the powers of the stomach and biliary system, effected a change in the irregular determina-

tion, which had formerly taken place to those parts. It was generally observed, where good effects did not follow the first exhibition of remedies of this kind, that harm was usually the consequence of a second. Vomiting, in short, was often rendered continual, and the distinction of paroxysm and remission was apparently destroyed, in consequence of the operation of violent emetics. Instructed by repeated examples of their hurtful effects, I at last scarcely ever employed antimonial vomits; even the safest kinds were used with caution. If it appeared, at any time, that the action of vomiting would be serviceable, camomile tea, or at farthest a few grains of ipecacuana were generally thought sufficient for the purpose. When this business was finished, a draught of cordial stimulating liquor, which had a tendency to promote a diaphoresis, was next administered. By this mode of treatment, especially if a blister was applied at the same time to the region of the liver, I have the satisfaction to add, that the bilious appearances for the most part vanished, and, if care was taken to support a determination to the surface, seldom ever returned during the continuance of the fever. Different seasons, and different situations of country were particularly distinguished by corresponding determinations. Thus a tendency to the bowels and biliary system was chiefly remarkable in the autumnal months, and in low and champaign countries; the head and breast were oftener affected in the winter months, and in hilly situations.

I observed before, that it is the first object in the cure of fevers, to remove those circumstances, or accidental states of the body, which hinder the disease from assuming its proper form. Thus, to procure remission, appeared universally to be the first business in the cure of the fever of Jamaica; the next, and a very important one, is to prevent the return of the paroxysm. If we knew a remedy, which

could be depended upon to accomplish this purpose with certainty, the cure of the disease would be easy; but the Peruvian bark, which almost infallibly stops the course of intermitting fevers in all countries, does not seem so indisputably to possess the same power over the usual endemic of the West Indies. I am sorry to own, that my knowledge on this subject, is not altogether satisfactory. At the time I left Jamaica I did not entertain a doubt, that the endemic of the district, where I resided, possessed something in its own nature which decidedly determined its duration. It was usually observed to terminate on a critical day, generally by very evident signs of crisis, and without seeming to be materially effected in its course, by the various and opposite modes of treatment, which were sometimes pursued. But though this was true for the most part, yet the superior efficacy of the very large doses of bark, which I afterwards ventured to give in the intermitting fevers of North America, obliges me to speak with hesitation, when I mention the powers of that remedy. It is probable that bark, with the same management, might have had the same effects, in the fever which prevailed at Savanna la Mar, as in that which is commonly epidemic in Georgia and the Carolinas; yet no doubt remains, that as commonly employed in Jamaica, it has no right to be considered as a remedy, which abruptly cuts short the course of the disease. In every case where it was tried, (except one) it did not seem to do more than give a degree of tone and vigour to the system, to excite a certain state of inflammatory diathesis, in consequence of which, the crisis was observed to be more perfect and complete, though it did not perhaps actually happen at an earlier period. Thus I am disposed to conclude, from all the experience which I have had, that bark is not generally carried farther, in the cure of the remitting fever of Jamaica, than merely to support the tone and vigour

of the powers of life. If we trust to it for more, in doses of two scruples or a drachm given every two hours, we shall certainly be deceived.

Having mentioned the different methods of treatment, by which it was attempted to procure remission in the endemic fever of Jamaica, and having likewise endeavoured to ascertain how far we can go in preventing the return of paroxysms, it only remains to detail some particulars in the management of the plan of cure, where the different species of fevers were distinguished by a peculiar train of symptoms. It was observed in general, that fevers, with a moderate degree of inflammatory diathesis, seldom required our interference. The disease, after a certain duration, terminated usually of its own accord. After I had gained some experience of the general course of fevers, I usually allowed those, in which I did not perceive marks of danger, to go on their own way, that I might better discover those periods, at which the disease was naturally disposed to terminate. Thus where the paroxysms continued regular and distinct, the remissions perfect, and the vigour unimpaired, nothing material was attempted to be done. On the contrary, where the paroxysms were long, or less distinctly formed, with signs which indicated an approaching affection of the nervous system, bark, and other remedies, which excited and supported the powers of life, were given with the earliest opportunity. Changes from inflammatory diathesis to nervous affection, were observed to happen frequently on the fifth day. Bark, and such remedies as imparted tone and vigour to the system, were given without delay; and the disease terminated for the most part on the ninth. In those fevers, which were of a complicated nature, in which signs of inflammatory, nervous, or putrid diathesis were variously mixed, blisters applied in different manners, opiates, bathing and antispasmodics were often materially useful; but it would

be arrogance to attempt to describe rules for the particular mode of application, which must vary more or less in almost every case, and which only can be learned from actual observation. There is one rule, however, in the treatment of fevers, of which the practitioner ought never to lose sight, viz. that wherever it is necessary to interfere, it is only the most vigorous decision which can do good. We cannot, as was said before, depend with certainty upon bark, as a remedy possessed of the power of absolutely cutting short the cause of the fever of Jamaica; yet wherever the fevers of that country discovered signs of nervous affection, I do not know any thing in the materia medica, from which such beneficial effects may be expected. If it did not actually stop the disease, it was eminently serviceable in conducting it to a favourable issue. Opium, wine, snake-root, &c. were often observed to heighten its good qualities: but the particular use of such additions can only be regulated by circumstances. Wine has been freely recommended in fevers with symptoms of nervous affection; and it must be owned, that its good effects were considerable, not only in real debility, but wherever the cause of the disease acted by weakening or depressing the powers of life. Wine was likewise observed to be more useful in cases of mobility and weakness, than in cases of stupor and suspension of the nervous influence. But though it is actually a remedy of great value, its virtues appear to have been greatly enhanced. In many instances it was not proper in any quantity; in some, it was only proper in a small quantity, and in very few, perhaps, could we allow of the quantities which are given in common practice. At one time I carried the use of wine in the nervous fever of Jamaica, to a very great length; but I afterwards learnt, that a third part of the quantity would have probably answered the purpose better. Though it undoubtedly is an useful cordial and tonic,

it is still inferior to cool air, and particularly to cold bathing.

In those treacherous and malignant fevers, which I have described in the third section of the sixth chapter, the ordinary medical aids were often feeble and insufficient. The course of the disease was generally rapid. There was little time left for deliberation; and where the first, at least where the second remission passed over, without some bold and decided steps to prevent the return of the paroxysm, or change the nature of the symptoms, the opportunity was probably never again in our power. The type of this malignant fever was usually of the single tertian kind; yet it was often found to anticipate, by such long anticipations, that the second paroxysm sometimes made its appearance, before any decided steps were taken by the practitioner to stop its progress, or often, indeed, before there were any surmises of danger. As this disease seemed to have a nearer resemblance to the intermittent, than some other species of the endemic of Jamaica; so bark appeared to be capable of producing more effect, in the present instance, than in those cases where the remissions were more obscure. It still, however, deserves to be remarked, that such scanty doses, as were usually given in Jamaica, could not be effectual; indeed, we could scarcely hope, that any powerful alteration could arise from less than half an ounce given at the short interval of every other hour. I confess candidly, that I never ventured so far; yet I also confess, that I am by no means satisfied with the success of the method of cure, which I adopted at first, in this disease. I lost some patients before I was aware of danger, and perhaps suffered others to die, from a dread of stepping over the bounds of common practice. At last, I acted with more decision; and have cause to be satisfied with the success of the attempt. As soon as I discovered the malignity of the disease, the marks of which, for the most

part, were plain in the course of the first paroxysm, the head was immediately shaved, and covered with a blister, which reached half way down the neck; the feet were likewise put into warm water, previous to the expected return of the paroxysm; the body was rubbed with brandy or rum; wine, and sometimes opium were given in quantity sufficient to exhilarate the spirits, or to produce a low degree of intoxication; bark was also prescribed in larger doses than usual, during the remissions; and cold bathing was occasionally employed with freedom. I do not say, that the return of the paroxysm was absolutely prevented by this method of treatment, but I have the satisfaction to add, that the fatal tendency of the disease was evidently obviated by it.

I must farther observe, that bark has been considered as the principal remedy in those fevers, the nature of which has been believed to be putrid; but the great fame of this remedy has proceeded from theory, rather than from actual observation. A real putrid disease, (as I have said before), if we except the yellow fever, occurs very seldom in Jamaica. Symptoms of languor and debility, however, are frequent in the fevers of that country; appearances which, though in fact, only signs of nervous affection, have often been imputed to a putrid tendency in the habit. Bark has been found to be efficacious in those cases of languor and debility, which have been falsely thought to arise from a putrefactive tendency in the system. Hence this remedy has gained credit, on a foundation which does not exist. I may add with truth, that the power of the bark was very equivocal, in those cases where the marks of putrid tendency were obvious. The principal dependence was then constantly placed in the cold salt-water bath; which, if well managed, produced most astonishing effects. An additional quantity of salt was often added to the water of the sea; and care was taken

that the coldness should be as great as the circumstances of the climate would permit. The chief dependence I have said, was constantly placed in this remedy, where the putrid tendency prevailed in the general system; but where it was more particularly confined to the bounds of the alimentary canal, saline draughts, in the state of effervescence, were useful, as were likewise glysters of cold water impregnated with fixed air. It is superfluous to mention cool air, clean linen, cold drink and the liberal use of wine.

The remedies, which I have hitherto mentioned, are such, as seem chiefly intended to support and invigorate the general powers of life; but which are less immediately directed to obviate the fatal tendency of particular symptoms; which I proposed to consider, as the second indication of cure. I shall probably depart materially from the usual ideas of practitioners in the prosecution of this subject. I do not deny, that bark may be given with safety in fevers, which are accompanied with local affections, or irregular determinations to particular parts; yet I must likewise observe, that bark is not the remedy on which the weight of the cure depends. Local affections, or irregular determinations are often distinguished by the name of the efforts of nature. I do not indeed dispute, where the part of the body, to which the determination takes place, is of little importance to life, or more certainly, where it is an organ of excretion, that such parts as are of greater consequence, are, in some degree relieved in consequence of this effect; and though there is still an impropriety, there is less danger in considering this irregular action of the morbid cause, as an effort of nature, or a quality of the *vis naturæ medicatrix*, by means of which, the health of the system is eventually rendered more secure. This, however, is only a circumstance of accident. It does not appear to depend upon a regular design of nature, and cannot

be admitted with safety as the basis of a plan of cure. We find, in short, that though the life of the whole is sometimes preserved by it, the destruction or derangement of the part is generally the consequence. But that the fallacious tendency of those tumultuary efforts of nature may be more clearly illustrated, I shall adduce the example of fevers, distinguished by an increased secretion of bile. It is an opinion, which seems to date its origin from Hippocrates, that bile vitiated in quality, or redundant in quantity, deserves to be considered as the cause of the species of disease distinguished by the name of ardent fever: and it must be confessed, that the frequent appearance of bilious discharges, in the fevers of hot climates, gives countenance to the supposition. From the frequency of this symptom, perhaps, the practitioners of the West-Indies adopted the idea, that bile is the cause of the fever of that country; while the method of cure, which they usually pursue, has served to confirm them in their error. Prepossessed with an opinion of the prevalence of bile, they administer cathartics and emetics with a liberal hand. If bile appears in the first evacuations, they consider it sufficient authority to proceed; if it does not appear, they conclude that the remedy has not been of sufficient force to reach the seat of the disease; and therefore persist in their intentions, till the effect is at last produced. It is well known, that a repetition of cathartics and emetics seldom fails to produce the appearances of a bilious disease. Hence this symptom of fever, and all the dangers which follow it, are frequently the work of our own hands. That this is the case, appears from a relation of the method of cure, which I usually adopted in such fevers as were distinguished by symptoms of this nature at an early period. Instead of encouraging the vomiting, or promoting the evacuation of bile downwards, I generally did every thing in my power to moderate, or even to check it.—

Sometimes I prescribed an emetic; but it was more with a view to excite the action of the stomach and biliary system, than to promote an evacuation of redundant or vitiated humours. After the operation of vomiting was finished, a blister was usually applied to the region of the liver, and such a plan of cure was pursued, as supported a determination to the surface of the body, and gave tone and vigour to the stomach and general powers of life. By this mode of treatment, bilious appearances vanished speedily, or ceased to be troublesome; while by the repeated use of emetics and cathartics, they generally continued long, and often prevailed throughout the course of the disease. We may thus, I hope, conclude, without any unnatural inference, that there appears to be danger in encouraging those tumults, which have been usually considered as the efforts of nature. If they are in fact efforts, no person can pretend to deny, that they are generally precarious; nay, that they are often the immediate causes of death.

I have now detailed the particular steps of the method of cure, which I adopted in the remitting fever of Jamaica; a disease which I treated, in some respects, on a different plan, and if self-love hath not blinded me with more success than the generality of those practitioners whom I had the opportunity of knowing, I treated the disease with success; but I dare not affirm, as some have done, that under this method of treatment, I never lost a patient. I proceeded, indeed, with diffidence and distrust of the powers of the medical art; venturing no farther than to support the general powers of life, and to obviate symptoms of a fatal tendency. Many pretend to cut short the course of fevers, by the force of a single remedy; but the means do not appear very obvious, and the effect was often precarious. I grant, that it is sometimes in the power of the practitioner to exterminate the cause of disease by forcible means, or

to destroy a certain aptitude of constitution, in which this disease may be said to consist; but I must at the same time observe, that there is danger likewise, lest he extinguish life. The bark, which has been so much celebrated for checking the course of fevers, though generally safe, is seldom effectual: others are frequently dangerous. During the time that I remained in the West Indies, I observed attentively the state of body, which usually attended recovery; as likewise those appearances which preceded, and apparently were the causes of death. Tone and vigour, or a moderate degree of the state of body distinguished by the name of inflammatory diathesis, without local affection, afforded the surest signs of safety; general failure of the powers of life, or irregular determinations to organs of importance, were the most certain appearances of danger. Thus, after obviating particular symptoms of a fatal tendency, it was the principal indication to support the general powers of life, or to excite the tone and vigour of the system. This was best accomplished by bark, wine, cool air, and above all, by cold bathing, which I am induced to consider as the most important remedy in the cure of the fevers of the West-Indies; and, perhaps, in the cure of the fevers of all hot climates. Though it might not absolutely cut short the course of the disease; yet it seldom failed to change the fatal tendency of its nature.

C H A P. XI.

YELLOW FEVER. (a)

THE disease, known in the West Indies by the name of yellow fever, particularly that species of the disease distinguished by black vomiting, has not, so far as I know, been described by the practitioners of any other country. In the autumnal fevers of most climates, indeed, as well as in the remitting fever of Jamaica, yellowness is not by any means uncommon: neither is vomiting of matter of a dark colour altogether rare, in the moments preceding death: yet in as much as I can judge, from what I have seen myself, or heard from others, the complaint which is the subject of the present treatise, possesses some characteristics of its own, different from those of every other disease. I do not pretend to determine, in what this characteristic difference precisely consists: yet I may say with confidence, that the species of this disease, which terminates in black vomiting, may be distinguished with certainty from the autumnal fever of aguish countries, or from the endemic remitting fever of Jamaica, even in the first hours of its attack. I must, however, at the same time own, that there is a species of disease, where the remissions are obscure, and where signs of nervous affection, or even sometimes of apparent inflammation, are more conspicuous than symptoms of putrescency, that I have some difficulty in classing properly. There is not any thing more foreign to my intention than multiplying names, or establishing distinctions which do not exist in reality; yet, as I have often seen instances of fever, to which yellowness and black vomiting did not seem to be essential; but in which paroxysms and remissions were extremely obscure,

or perhaps did not exist, I at last ventured to conclude, that this species of illness had more affinity with the disorder usually known by the name of yellow fever, than with the common remitting endemic of the country. I shall therefore describe it as a species of that disease; though I am less solicitous about fixing its place in nosological arrangement, than of giving a history of it, by which it may be easily recognized.

I am not ignorant that there are several authors, who have written on the subject of the yellow fever; yet I had not the opportunity of consulting any of them, except Dr. Hillary, during the time that I lived in Jamaica. This writer's method of cure was adopted very generally by the medical people of the island, and many of them were disposed to think favourably of its success. I cannot however conceal, that I was disappointed in every instance where I saw it employed. If, in any case, the patient recovered, this fortunate event appeared to be more owing to great natural strength of constitution, or to a lower degree of disease, than to the efficacy of the method of treatment. But besides, that this author's method of cure is feeble and ineffectual, the historical part is particularly defective; the disease, in short, being only very imperfectly discriminated from the common endemic of that country. The consideration of these circumstances, induced me to throw together the observations which I had made on this subject, during the time that I resided in the West Indies; and though I am conscious that they are imperfect, I still hope that they contain some hints which may be useful to those, who have had little experience of the fevers of hot climates.

In our enquiries into the history of the yellow fever, some circumstances present themselves to our observation which are not a little curious. It has never been observed that a negro, immediately from

the coast of Africa, has been attacked with this disease; neither have Creoles, who have lived constantly in their native country, ever been known to suffer from it: yet Creoles or Africans, who have travelled to Europe, or the higher latitudes of America, are not by any means exempted from it, when they return to the islands of the West Indies. Europeans, males particularly, suffer from it soon after their arrival in the tropical countries; yet, after the natives of Europe have remained for a year or two in those hot climates, especially after they have experienced the ordinary endemic of the country, the appearance of the yellow fever is observed to be only a rare occurrence. But besides, that this disease seldom discovers itself among those people, who have lived any length of time in a tropical country, it has likewise scarcely ever been known to attack the same person twice, unless accidentally after his return from a colder region. The remitting fever, on the contrary, does not cease to attack such as have resided the greatest part of their life in those climates; or who have lived after the most regular and abstemious manner; a fact which seems to prove, that there actually exists some essential difference between the two diseases; or which shews, at least, that the revolution of a season or two destroys in the European constitution, a certain aptitude or disposition for the one disease, which it still retains for the other.

Having thus premised some circumstances, which regard the general nature of the yellow fever, I shall proceed to give a more particular description of the disease, previously dividing it into three forms, in each of which, I believe, I have frequently seen it appear. 1. Into a species of disease, in which signs of putrefaction are evident at a very early stage, which is generally rapid in its course, and which usually terminates in black vomiting. Yellowness seldom or never fails to make its appearance in the present in-

stance ; and perhaps it is only this form, which, strictly speaking, can be called the yellow fever. 2. Into a form of fever, which either has no remissions, or remissions which are scarcely perceptible ; in which signs of nervous affection are more obvious than symptoms of putrescency ; and in which yellowness and black vomiting are rare occurrences. 3. Into another form, in which regular paroxysms and remissions cannot be traced ; but in which there are marks of violent irritation, and appearances of inflammatory diathesis in the earlier stage, which give way after a short continuance to signs of debility and putrescency, to which yellowness frequently succeeds, or even sometimes the so much dreaded vomiting of matter of a dark colour. The disease, which I have divided in the above manner into three distinct forms, appears to be in reality only one and the same. The difference of the symptoms probably arises from very trivial or very accidental causes ; and it is a matter of great difficulty to discriminate those signs, which are essential and necessary to its existence. It is in some degree peculiar to strangers from colder regions soon after their arrival in the West Indies, and may generally be distinguished from the remitting endemic of the country, not only by the obscurity, or total want of paroxysms and remissions, but likewise by a certain expression of the eye and countenance, with something unusually disagreeable in the feelings, of which words convey only an imperfect idea.

SECTION I.

I SHALL describe, in the first place, the most common and most formidable species of this disease, which, as I observed before, is distinguished by early signs of putrescency, by an intense degree

of yellowness; and, towards its termination, usually by vomiting of matter of a dark colour. It was mentioned in the preceding treatise, concerning the remitting fever of Jamaica, that fevers of different types had their different hours of invasion; but no such property was observed in the present disease. In some instances the yellow fever began in the morning, though the evening, upon the whole, was the more usual time of its attack. The first symptoms were languor, debility and head-ach, together with an affection of the stomach peculiarly disagreeable. This last often preceded the others, and was in some measure characteristic; but it is impossible to give a clear idea of it in words:—anxiety, nausea, and certain unusual feelings were so strangely combined, that any description, which I might attempt to give of this complicated sensation, would hardly be intelligible. The horror and shivering, which so usually precede fevers, was seldom great in degree in the present instance; but it sometime continued long, and was often accompanied with sensations of a very unpleasant kind. The heat of the body, though rarely intense, was frequently of an acrid and pungent nature. The pulse was weak and confined in its stroke. It was likewise frequent, and the nature of the arterial pulsations were creeping or vermicular; in short there was a perpetual motion under the finger, combined with something, which gave the idea, that the disease was not of the kind which has paroxysms and remissions. Together with this, the eye was sad and watery; or in some degree inflamed, having much that appearance, which is the consequence of exposure to the smoke of green wood. The face was often flushed; yet the flushing, in the present case, was different from that which arises from ordinary causes. There was a degree of confusion, and often a degree of grimness joined with it, difficult to be described in words; but which a person, ac-

quainted with the appearances of the disease, immediately recognizes as a distinguishing mark of its character. The tongue was often moist, and generally foul; the thirst was seldom great, and though there was usually a peculiar nausea, there was rarely any severe vomiting or retching. The breathing was hurried, for the most part, with much anxiety and distress; while the patient frequently expressed sufferings, which a person, unacquainted with the nature of the disease, would be disposed to believe were not real.

The symptoms, which I have enumerated above, are those which usually shew themselves in the first twelve hours of the disease. I marked them with all the attention of which I was capable; yet still I am fearful, that the history may not be so explicit as to be totally free from ambiguity. The characteristic marks of the yellow fever, are not by any means doubtful to a person well acquainted with the diseases of hot climates; but they are not easily conveyed in words, and may often be overlooked by those, who have drawn their information from books alone. I am induced to think so, from an instance which happened to myself. I had read Hillary's account of the yellow fever, both before and soon after my arrival in Jamaica; I had likewise heard some conversation on the subject, so that I might be supposed to have been tolerably well informed of the general character of the disease; yet the first person, who came under my care in this illness, was within a few hours of death before I knew the disorder, or even suspected it to be of a dangerous nature. Fortunately for the peace of my conscience, the patient had been visited, on both the first and second day after the attack, by a practitioner who had lived many years in the island; but, between carelessness and inexperience, the poor man's situation was either not known, or not attended to, till approaches of death were visible. The body

had been evacuated very plentifully by a solution of salts, during the two first days of the illness ; but no material good seemed to ensue from it. The patient complained still more on the second day, than he had done on the first ; but as the external signs of fever were moderate, I really suspected that he complained without much cause. It so happened, that I could not visit him on the third ; and on the morning of the fourth, he became of a deep orange colour, and vomited black matter in great quantity. I then suspected, that this complaint, to which I had not paid particular attention, was actually the disease known by the name of yellow fever ; but it appeared likewise, to be so far advanced in its progress, that I could do nothing more than witness the approach of death. My want of discernment, and in some degree my carelessness, a charge from which I cannot altogether acquit myself, made so deep an impression on my mind, that I turned over every circumstance of the disease with which my memory supplied me ; and I soon had that satisfaction to find, that the mistake which I had committed, had not happened to me without leaving an useful lesson. In ten or twelve days another person was affected in a manner so similar to the former, that I suspected the disease to be the same, and the event proved my suspicions to have been well founded. From that time forward, I never found difficulty in distinguishing this particular form of fever, in the first hours of its attack, not only from the remitting endemic of the country, but even from the other two species of this disease, which I shall afterwards describe.

A trifling abatement of the symptoms, was sometimes taken notice of, in ten or twelve hours after the commencement of this disease ; but in no instance so far as I have observed, was there ever so much alleviation, as with any justice could be called a remission. The symptoms of distress, where any abate-

ment had been perceptible, recurred in a short time with aggravation; and if there actually ever was any relief afterwards, it was only momentary and uncertain. The appearance of the eye became still more desponding, with a sensation of burning heat, and greater marks of inflammation, affording incontestible signs of the real genius and nature of the fever. The pain of the head was now violent; the countenance was confused and grim: the gums were frequently spongy, and disposed to bleed; the tongue, which was sometimes moist, sometimes dry, was almost always foul; the thirst was irregular; at one time intense, at another very little increased beyond what it naturally is. Nausea, I observed above, was a common sign, from the first hours of the illness; yet vomiting, during the first day or two, was not by any means a constant, perhaps scarcely a frequent symptom: neither, if it did take place, was it often found to be bilious. The liquor thrown up, for the most part, was clear; in short, seldom altered from the state in which it had been drank, unless by having acquired an unusual degree of ropiness, or by presenting some flakes of a darker coloured mucus. To the above symptoms we might add, uncommon restlessness and anxiety; a torment scarcely to be expressed in words; watchfulness; a hurried and difficult respiration; frequent deep and heavy sighing, with more or less disposition to faint, where any exertion was attempted. It deserves to be remarked, however, with regard to the disposition to faint in the yellow fever, that it did not depend upon the same cause, as in some other diseases. It seemed, in fact, to be owing to torpor of the nervous power, rather than to excess of mobility. The patient was often able to stand upright, for some time, even to walk to a considerable distance; and when at last overcome, was observed to fall down in a torpid, rather than in a fainting state. Sweating was a rare occurrence in

this stage of the disease ; at least I do not find, that I ever had remarked any greater degree of it, than a clammy moisture on the head and neck. It was also rarely observed, that the external heat was much increased beyond its natural state ; while the pulse now began gradually to abate in point of frequency. The yellowness, which is intense in the last stage of the disease, was seldom seen in the period which I now describe ; yet, together with a general obfuscation of countenance, a tawney hue rather than a paleness, was observed about the eyes and corners of the mouth, when the patient turned accidentally languid and faint. The body was frequently costive in the first days of the illness ; and I have even seen some instances where strong cathartics did not occasion the usual evacuations. The urine was generally high coloured, and turbid. In some cases there was active hæmorrhage from the nose ; which was generally followed with some relief from the violent pain of the head. I have also observed a high degree of delirium, though I never saw any instances, where this symptom was of long continuance.

The duration of the tumultuary state, which I have described is uncertain. Sometimes it did not exceed twenty-four hours, though it more generally continued till the third day ; sometimes even longer. The symptoms, which now made their appearance, were many of them different in their nature from the former. The agony of distress, which was so strongly depicted in the countenance of the patient, during the first days of the disease, was observed about this time to be sensibly diminished ; the eye became more cheerful, the countenance more serene and composed ; yet yellowness of the skin became speedily evident : the external heat and fever subsided ; the pulse became gradually fuller and slower, and approached by degrees to its natural state : no sweat or moisture was now observable on any part of the body ; the state of

the skin impressed the idea, as if it were not pervious to any degree of perspiration, and heat gradually forsook the surface and extremities ; the tongue turned moist, and at the same time frequently clean about the edges : the gums turned redder, more spongy, and shewed a greater disposition to bleed : vomiting was now troublesome—the liquor thrown up was ropy, much in quantity, and abounding with villous or mucus flakes of a darker colour : thirst in a great measure vanished ; but sensations of anxiety, distress and uneasiness in the region of the stomach suffered no material abatement.

Things went on in this manner, sometimes for one day only, sometimes for two, three or more. The circulation in the extreme vessels became gradually more languid ; the natural heat retired from the surface of the body, which was now dry and impervious ; the pulse returned nearly to its ordinary state, or became slow, full, and regular ; the yellowness increased fast ; so that the whole of the body was frequently yellow as an orange, or of as deep a colour as the skin of an American savage ; anxiety was inexpressible ; vomiting was irresistible, and the so much dreaded symptom of vomiting of a matter resembling the grounds of coffee, at last made its appearance. It deserves, however, to be remarked, with regard to this formidable symptom, that the colour of what was thrown up, was often black as soot, where the disease had hurried on rapidly to the last stage : while it was not only less intensely black, but was often tinged with green, where the progress had been slow and gradual. I observed before, that villous or mucus flakes were discovered early in the vomitings of the patient, and that these appearances increased as the disease advanced in its progress. I may now add, that streaks of blood were sometimes found to be joined with them ; the greatest part of which seemed to come from the throat and gums. The

vomiting, which now returned at shorter intervals as the disease approached this fatal period, was seldom accompanied with violent retching. A quantity of liquor, sometimes a quantity so enormous, that we could not help wondering whence it had been supplied, having been collected in the stomach, was discharged without much difficulty, and the patient enjoyed some respite till a like accumulation had again taken place. It may further be remarked, that as soon as the vomited liquor acquired this dark and foety colour, the belly generally turned loose, the stools being black and smooth, not unlike tar or molasses. The tongue likewise became clean, the gums became putrid; hæmorrhage, or rather oozings of blood were sometimes observed at different parts of the body; while livid blotches frequently made their appearance on the belly and insides of the thighs. The pulse, which during the latter stages of the disease, could scarcely be distinguished from the pulse of a person in health, became at last quick, irregular, or intermitting; soon after which, coma or convulsions closed the scene. It may not be improper to remark, before leaving the subject, that the yellowness of the skin, which was said to precede the black vomiting in most instances, in some cases was found to succeed to it. In such, the vomiting began unexpectedly, or without much previous affection of the stomach; the colour of it was usually intensely black, the patient turned yellow almost in an instant, and died in a very short space of time:—the disease, in short, passed suddenly from the first stage to the last.

I may observe in this place, that the number of those who recovered from the last stage of this species of the yellow fever, was extremely small: yet, though such fortunate instances were rare, they were not altogether wanting. The termination, however, did not appear to be by regular crisis. The black vomiting ceased, sometimes apparently in consequence

of treatment, sometimes evidently of its own accord ; but a vomiting of a ropy, glutinous matter continued for a great length of time, together with an extreme irritability of stomach, and a very peculiar state of the skin ; which sometimes did not recover its natural smoothness and unctuousity, till after several weeks had elapsed.

During the time that I lived in Jamaica, I opened several persons who died of this disease ; but it was seldom that I found any material variation in the appearances. Soon after death, and even sometimes before death had actually taken place, the body became covered with large livid blotches ; and, it is almost unnecessary to mention, was extremely offensive. In opening the abdomen, the omentum and all its appendages were discovered to be in a dry and parched state, and of an uncommon dark grey colour. But together with this dark grey colour of the omentum, and a want of the unctuousity or moisture, which is usually found in the cavity of the abdomen, the stomach and intestines had a dirty yellow appearance, were highly putrified, and much distended with wind. The liver and spleen were generally enlarged in size ; the colour of the liver was often of a deeper yellow, than that of any other of the abdominal viscera ; while the texture of the spleen was frequently less firm, than it is found to be in its natural state. The gall-bladder, for the most part, was moderately full ; but the bile it contained, was black and thick, not unlike tar or molasses. The biliary ducts were likewise enlarged, and moderately filled with the same sort of bile, which was found in the gall-bladder : while the very blood vessels of the liver bore the marks of uncommon distension. In the cavity of the stomach likewise, there was usually more or less of a dark coloured liquor, similar to what had been thrown up in the last stage of the illness. But besides, that this dirty fluid was generally present in the stomach

in considerable quantity, the villous or inner coat of that organ was also abraded in various places; at the same time that some spots appeared on the surface, which were probably the beginnings of mortifications. The superior portions of the intestinal canal were generally in a situation similar to what I have described; only it must be remembered, that the morbid appearances were not yet so far advanced in progress.

The state of the body, as it appeared on dissection, throws considerable light on the nature of the yellow fever. It enables us to explain satisfactorily many of its leading symptoms; and may even afford useful hints in the conduct of the cure. It was mentioned above, that the natural heat and vigour of circulation retired from the surface and extremities of the body at a certain period of the disease; and that a copious and obstinate vomiting ensued soon after this change had taken place. The fluid thrown up, which was usually pituitous, glutinous, or flakey in the beginning, acquired, after some time, a colour of various degrees of blackness. In quantity, it was often immoderate, bearing no proportion to the liquor which was drank; a circumstance which could only be explained by the ordinary determination to the surface of the body being turned upon the internal parts; in consequence of which, there was a preternatural discharge of fluid into the cavity of the alimentary canal. Flakes, of a mucus or villous nature, were likewise frequently observed in those matters which were thrown up by the patient; an appearance which we could not have easily accounted for; unless we had discovered, in examining the dead body, that the inner coat of the stomach was actually abraded; but in what manner this happened, may be difficult to explain. It might either arise from the repeated action of severe vomiting; or, still more probably, from the preternatural and forcible determination to the exhaling vessels of this cavity, forcing

off some portions of the villous coat, in the manner of cuticular blisters. To which explanations I may add, that the black colour of the vomited matter, was evidently owing to a mixture of vitiated bile ; the passage of which might be easily traced from the gall duct into the pylorus.

The species of the yellow fever, which I have now described, is universally acknowledged to be a terrible disease ; and there are few, I believe, so uncandid, as to boast of general success in the manner of curing it. A road is therefore left open, not only for improvement, but almost for total innovation. The only author I have read on the subject, or the practitioners with whom I am acquainted, do not seem to have extended their views beyond the symptoms of the disease. There are some, who, from observing that there is pain of the head and flushing of the face, recommend bleeding ; others, from the presence of nausea or inclination to vomit, make trial of emetics ; and many, from various causes, insist on the indispensable use of cathartics. My views, I must confess, are different from those of preceding authors. Bleeding was employed occasionally ; emetics were cautiously avoided ; but time appeared to be too precious to be spent in attending to the effects of cathartics, which cannot often be known in less than twenty-four hours ; and which at best are precarious or feeble. Instead, therefore, of attempting to evacuate redundant bile, or to correct it when supposed to be vitiated, I exerted myself, from the first moment that I was called to the patient, to change the genius and natural tendency of the disease ; or, if I may be allowed the expression, to take the business, as speedily as possible, totally out of the hands of nature.

I remark in the first place, that I generally began the cure of this species of the yellow fever with bleeding. Bleeding was employed in the present case, chiefly with a view of paving the way to remedies of

greater efficacy. It was, however, found to moderate the violence of local pain, particularly the violence of the head-ach, and to be not altogether without effect, in retarding the usual rapid progress of the disease. It has hitherto been thought necessary, indeed almost indispensable, to empty the first passages in this species of fever; but time is short, and the good which accrues from such evacuations, is not very certain, and often not essential. It was, therefore, thought sufficient to trust this intention, for the most part, to laxative glysters; after the employment of which, (bleeding having been premised in such quantity as was deemed proper,) the patient was washed clean, and bathed in warm water, in as complete a manner as the circumstances of situation would permit. It is needless to mention, that this was done with a view to increase mobility of system, and to remove spasmodic stricture from the extreme vessels of the surface; in consequence of which, greater benefit was expected from the application of cold salt-water, which was dashed suddenly from a bucket on the head and shoulders. This practice may appear hazardous, to those who argue without experience; but I can vouch for its general safety, and bear testimony to its good effects. Sweat, with perfect relief from all the feelings of anxiety and distress, was generally the consequence of this mode of treatment. If employed within the first twelve hours from the attack, it seldom failed of removing all the symptoms of danger: or of effecting a total and complete change in the nature and circumstances of the disease; but if the progress was more advanced, though the same rule of practice might still be proper, the execution required more boldness and decision. It is only possible to judge from the circumstances of the case, at this period, of the necessity or propriety of bleeding, and of emptying the lower intestines by means of glysters; but when this business shall have been ac-

complished, in such manner as may be deemed right, or conducive to the main view, it will be adviseable to shave the head, to bathe the whole body in warm water, and instantly to dash cold water from a bucket on the head and shoulders. I have even sometimes, where there was an appearance of greater obstinacy, ventured to wrap the whole body in a blanket soaked in sea water, or water in which was dissolved a large portion of salt. If anxiety was great, or nausea and vomiting troublesome, I have also observed benefit from the application of a blister to the epigastric region. Opiates, joined with remedies which had a tendency to determine to the surface, were found to be serviceable; and wine, with a supply of fresh and cool air, in most cases, was highly necessary. This method of proceeding will, perhaps, be thought unwarrantable; but I can speak confidently of its safety; and I may farther add, that unless some decided steps are taken to change the nature of the disease, during the continuance of this stage, our future endeavours to do good, will generally be in vain. I have hitherto promised success in the cure of this fever, with a good deal of confidence; but if it should so happen, that we are not called to the patient till the yellowness has spread over the whole of the body, or till the black vomiting has begun to make its appearance, the prospect, I must confess, is then very dark. The ordinary resources of our art are feeble; and if good can be done at all, it can only be done by means, which in the common opinion of practitioners, border on rashness. In this latter stage of the complaint, so great a degree of torpor overwhelms the powers of life, that remedies do not produce their usual effect, and our labour is often the same, as if we attempted to resuscitate a corpse. I have, however, seen instances of such unexpected recoveries from the most hopeless state in fevers, that I seldom totally despair as long as life remains. I know that

death may be prevented, even after black vomiting has appeared with all its terrors, if a remedy can be found powerful enough to excite the action of the extreme vessels, and to recall the determination to the surface of the body. For this purpose, I have employed alternately warm and cold bathing with success. I have even wrapt the body, as I mentioned before, in a blanket, soaked in water, in which a large portion of salt was dissolved, or which had been steeped in brandy or rum, enjoining at the same time the liberal use of wine, or even more powerful cordials. I have heard of some well-attested instances, where plentiful draughts of rum and water, have checked the vomiting, and apparently saved the lives of patients, after the medical people had given them up for lost.

I have now mentioned the method of cure which I pursued in the yellow fever of Jamaica; and I must be allowed to add, that the general indication appears to be confirmed by a view of the history and progress of the disease, as also by considering the appearances which are found after death. It was observed in the preceding pages, that the circulation became languid at a certain period in the course of this fever, and that the determination was, in fact, turned upon the internal parts, particularly upon the alimentary canal, and biliary system. To support, therefore, or to recall the determination to the surface, where it had begun to retire, was the principal aim which was kept in view. It was pursued with vigour; and, I have the satisfaction to add, frequently with success. I am afraid that the means may be thought hazardous; but I have never yet perceived from them, even a momentary harm. I shall not therefore cease to recommend them, till I find that others have tried them fairly, and found them dangerous or ineffectual.

SECTION II.

IN the preceding pages, I attempted to describe the disease, which has been usually regarded as the proper yellow fever of the West Indies, detailing at the same time, the particular steps of a method of cure, which I have cause to believe, was followed with more than ordinary success. I now proceed to consider another species of disorder, which frequently makes its appearance among people newly arrived in hot countries, and which, from some striking marks of affinity, I have been induced to rank as a species, or variety of the former. Yellowness, indeed, is not by any means common to it, and black vomiting is actually rare; yet paroxysms and remissions are scarcely distinguishable, and the difference between it and the preceding, is perhaps, in fact, only accidental.

I remarked before, that this species of disease, as well as the yellow fever, properly so called, appears but rarely among those who have resided any length of time in tropical climates. It was observed to begin, as fevers usually do, with disagreeable affection of the stomach, with languor, debility, and pain of the head. The horror of shivering, so common in the commencement of febrile diseases, was seldom great in degree; but it often lasted long, and sometimes was accompanied with unusual feelings. The pulse was generally small, frequent, and easily compressed; the eyes were watery, muddy, or inflamed; the features were confused, and the countenance was sometimes flushed: the thirst was seldom great; and the heat of the skin was usually moderate; but a deep and heavy sighing, a hurried respiration, with an inconceivable distress and anxiety about the præcordia, gave strong indications of the nature of the complaint.—In some instances I have known such

fevere and excruciating spasms, as, in some measure seemed, to suspend the ordinary functions of life.

In twelve hours, or less, there was often some abatement in the violence of those symptoms; but seldom such material relief, as, with any degree of justice, could be called a remission. The skin became cool, and sometimes moist; yet there scarcely ever was any sweat. The pulse became fuller, and often less frequent; the restlessness and anxiety were sometimes sensibly diminished; and the local pain often abated; but this respite was neither long, nor of certain duration. In a few hours, all the symptoms returned with aggravation. The eyes became more muddy; the countenance more confused; the head-ach, and other pains increased, together with sensations of anxiety, and restlessness, hurried respiration, and deep and heavy sighing. The pulse was now more frequent, smaller and harder; the thirst was increased, with nausea, and sometimes with vomiting. The vomiting was seldom bilious: it was not often, indeed, that the matter thrown up, was altered from what had been drank, unless by having acquired an additional degree of ropiness.—To the above symptoms was sometimes added an obstinate costiveness, sometimes such a degree of purging and griping, as might easily be mistaken for proper dysentery.

As the disease advanced in its progress, the abatement of the violence of symptoms, which at first was sometimes perceived towards the mornings, became gradually less and less perceptible, and at last was scarcely to be distinguished. The anxiety and restlessness were now particularly distressing; the skin was sometimes dry, though oftener moist, and in point of heat below the ordinary temperature of health; while it gave the idea to the person who felt it, as if there was a powerful spasm subsisting on the surface. I may likewise remark in this place, that a beautiful red colour of the cheeks, together with a smooth-

ness and cherry plumpness of the lips, was frequently observed towards the latter periods of the disease. Yellowness, as was mentioned before, was seldom seen, unless in the very last stage of the illness; and vomiting of black, or even bilious matter was extremely rare. There was, however, at all times, a great disposition to faint, with more or less of a certain species of low delirium.

The course of this species of the disease, was less rapid, than the course of that which terminates in black vomiting; the termination of the one being often protracted to the eighth or ninth day, that of the other seldom exceeding the fourth or fifth. The marks of crisis, as was observed before, were rarely discoverable in the first species of the yellow fever. They were likewise obscure in the present, and I cannot pretend to speak with confidence, of the influence of critical days. Where the termination was favourable, the pulse became gradually stronger, and less confined in its stroke; the skin likewise became softer, while the impression, which it made on the hand that felt it, communicated an idea that the circulation was more vigorous, and the spasm on the surface less obstinate; the eye and countenance likewise brightened up; the anxiety and restlessness vanished or decreased, and some appetite for food returned; but it was often difficult to mark the point of time precisely, at which this change took place.—It may be observed likewise, where the termination was fatal, that death approached in two different ways. A patient, apparently possessed of vigour, was sometimes suddenly seized with coma or convulsions, and died unexpectedly; but it happened more frequently, that the powers of life were gradually and slowly extinguished; the pulse became weaker and more confined in its stroke; while the natural heat and circulation retired by degrees from the surface and extremities of the body.

The cure of this species of the disease, though by no means easy, was less difficult upon the whole than that of the former. Instead of the torpor and insensibility, which prevailed in the latter periods of the proper yellow fever, the mobility of the nervous system was so much increased in the present species of disease, that remedies seldom failed of producing sensible effects: and wherever remedies produce effects, it generally is in our power to manage the business in such manner, that some good may arise. It may be observed in the first place, with regard to the cure, that bleeding, which frequently was useful in the former species, was generally hurtful in the present; and that instead of retarding, it oftener accelerated the progress of the disease. Emetics were employed very commonly by the practitioners of the West Indies, in this as in other cases of fever; but I cannot help remarking, that languor and debility, frequently yellowness, and sometimes a continual vomiting, which no remedies could restrain, were often the consequence of antimonial emetics of severe operation; and I have no doubt in saying, that the approach of death was actually hastened, in several instances, by this method of treatment. Laxatives were occasionally of service; but the stronger purgatives were frequently hurtful. Blisters were often extremely beneficial; but it requires care and discernment to apply them in the proper circumstances, so as to reap the full advantage. Opiates were sometimes serviceable, and bark and wine, in most instances, were remedies of great value; but the principal trust was placed in warm and cold bathing; which, under proper management, seldom failed of answering every expectation completely, or speedily, of removing the chief symptoms of danger. Sometimes it appeared to cut short the course of the disease abruptly.

SECTION III.

I HAVE now described two species of a fever, which seems to be, in some degree, peculiar to the natives of northern regions, soon after their arrival in the West-Indies. In the one, a determination to the alimentary canal and biliary organs, with marks of putrescent tendency in the general mass of fluids, was discoverable at an early period; in the other, the brain and nervous system were more particularly and principally affected; while the species, of which I now attempt to give some account, exhibited strong marks of vascular excitement, with a very high degree of the apparent inflammatory diathesis. This was more irregular in its appearances and more complicated in its nature, than the others. The marks of inflammatory diathesis were generally very apparent in the beginning; but they usually gave way or became complicated in the latter stages, with symptoms of putrescency or nervous affection. In describing the history of this disease, it may not be superfluous to remark, that there is seldom any thing particular in the sensations of debility and horror, which precede the formation of the paroxysm. The hot fit was generally observed to run high; the heat was often intense; the pulse, which was quick, frequent and irregular, vibrated often in an uncommon manner, and with an usual degree of force; the thirst was sometimes immoderate, sometimes not greatly increased; the countenance was flushed; the eye glistened, and appeared frequently to be in some degree inflamed; the signs of excitement were in general uncommonly high; yet the disposition to faint was sometimes sudden and unexpected. It deserves farther to be remarked, that blood drawn from the arm did not commonly exhibit the usual buffy appearance of real inflammatory

diathesis; and though times of aggravation and alleviation were often discernible; yet they did not happen at regular and stated periods.

It was observed frequently, that many of the leading circumstances suffered a material change, about the third day of the disease. The symptoms of high inflammatory diathesis, which prevailed in the beginning, became mixed, more or less, with symptoms of putrescency, or nervous affection. Delirium made its appearance; sometimes it ran high, with startings and symptoms of violent excitement; sometimes there was a low and muttering incoherence with marks of languor and debility. The gums turned red and spongy, and sometimes bled; the thirst was frequently intense, the tongue dry, with vomiting and severe retching; yet vomiting of bilious or vitiated matters was a rare occurrence. The above symptoms generally went on to increase, during the space of six or seven days, about which period the powers of life either yielded to the disease, or signs of recovery began to appear: the marks of crisis, however, were seldom distinct and final; neither was the influence of critical days so much to be depended upon as in the common remitting fever of the country.

It was mentioned above, that the nature of this fever was more complicated than that of the two former; so the indications of cure are likewise more difficult and perplexed. If we proceed on the first obvious view of the disease, we shall often do irreparable mischief by copious and repeated evacuations; yet there will not be less danger, on the other hand, if, regardless of the present degree of excitement, we indulge freely in the use of stimulants. It is necessary to observe a middle course; and I must confess, that it is sometimes difficult to do any thing, without doing harm. Bleeding was frequently employed in the cure of this disease, and in most cases, it was a useful remedy, though less perhaps from its own

effects merely, than from paving the way to other more powerful applications. It is, however, capable of being easily carried to excess; and ought not to be trusted to wholly for the removing of the irritability, and high degree of excitement, which prevails so generally in the beginning of this disease. After bleeding, emetics and cathartics are employed very freely. I have always professed myself an enemy to the practice of giving emetics in the fevers of Jamaica; yet, I must confess, that antimonials were not only safer, but of more particular service in this, than in any other species of fever, where I have seen them tried. Among the great variety of forms which have been recommended by practitioners, for the purpose of emptying the first passages, I have not found any one answer so well, as a thin solution of the sal-catharticum, given at different intervals, with a small portion of emetic tartar, and sometimes with the addition of laudanum. The operation of this remedy was extensive. It might be so managed, as to promote nausea or vomiting, sweat, or moderate evacuations downwards; at the same time that it proved very powerfully sedative. I may likewise add, that I have sometimes found benefit from nitre, camphire and opium, given in pretty large doses, and accompanied with plentiful dilution. But though these remedies were often serviceable, and contributed in many cases to moderate the high degree of irritability; yet the chief dependence of the cure was much better trusted to cold bathing. After the surface of the body had been sufficiently relaxed, by the previous use of warm bathing and fomentations, the effects of cold bathing were wonderful. The excessive irritability was moderated or removed, and the powers of life were invigorated in a very singular manner in consequence of it.

I have attempted in the preceding pages, to give a short view of a disease, which has not, I believe,

been hitherto very accurately described by authors, or treated with much success in practice. It is a disease of a continued kind; and, as I said before, in some degree peculiar to the natives of northern latitudes, soon after their arrival in the tropical climates. I cannot help thinking, that it may be easily distinguished, even in the first hours of its attack, from the intermitting or remitting fever, which is the common endemic of hot countries: but I must at the same time add, that this distinction does not reside in the presence or absence of one individual symptom. The state of the pulse, indeed, conveys information, that the disease is not of the kind which has paroxysms and remissions: yet this information can only be obtained from a knowledge and actual comparison of the two diseases:—I do not pretend to describe it in words. The state of the eye and countenance, was likewise observed to be strongly descriptive of the nature of the disease; as also were the deep and heavy sighing, the hurried respiration, the anxiety and restlessness, with a certain uncomfortableness of sensation, which no words can express; but I confess myself, at the same time, perfectly at a loss to fix on any one single symptom, which appearing at an early period, discriminated it with certainty from all other fevers. I have described it under three distinct and separate forms; but I must also add, that they may sometimes be found to be more complicated with each other, than they appear to be in the above description.

C H A P. XII.

INTERMITTING FEVER OF AMERICA.

HAVING endeavoured in the preceding treatise, to give a more accurate history of the endemic fever of Jamaica, than is met with in books, and I am disposed to flatter myself, having pointed out a more successful method of cure than that which has been generally pursued; I shall now add a few observations on the intermitting fever of America; a disease, in which my experience has been tolerably extensive. The frequent occurrence of intermitting fevers in every climate, together with the full and ample manner in which the disease has been treated of by many learned and ingenious writers, excuses me from entering into a minute and full discussion of the subject. I shall therefore employ only a few pages in attempting to illustrate particulars in the history of the disease, which have been superficially noticed; or to explain some points of treatment, which, though not new, I have ventured to carry farther than is usual in common practice. As I had the opportunity of attending to the history of the intermitting fever in several of the southern provinces of the continent of North America, I shall first mention the more constant and general course of the disease, and afterwards point out those circumstances of peculiarity, which seemed to arise from the difference of climate, or from the influence of the season of the year. I shall likewise occasionally take notice of the general state of health of the troops who were employed on the same expedition,

though I must also add, that I can only pretend to trace the progress of the fever with accuracy, in the regiment in which I had the honour to serve.

I shall attempt, in the first place, to give an accurate description of the paroxysm of an intermitting fever, marking as carefully as I can, the order of succession, in which the symptoms most usually appear. We are taught by the descriptions of most writers, to consider languor and debility as the first feeling or first essential symptom in the paroxysm of an intermitting fever; but I cannot avoid remarking, that an unusual affection at stomach, a flatulence,—in short, something disagreeable, which I cannot easily define, but which was accompanied in many cases with head-ach, and sometimes with drowsiness, preceded every sensation of languor or debility in most cases, where my observations were made with such care that they could be trusted to. I may also farther observe, that, as soon as this languor or debility began to be perceived, the veins began to subside, the nails turned pale, and at last blue; the skin of course was dry and constricted; and there was sometimes an evident diminution of heat, particularly of the heat of the extremities. To these symptoms was often added, a disagreeable kind of yawning, with strong sensations of weariness, and an irresistible inclination to stretch the limbs. A sensation of cold was now felt in the back, as if water ran down upon it in separate streams. It soon vanished, indeed; but suddenly returned again in a more violent degree; in which manner it went on, ceasing for an instant, and then recurring with aggravated violence, till the whole body became at last affected with rigour or shaking, accompanied, in a more especial manner, with chattering of the teeth. The coldness having now arrived at its acme, or highest point of intensity, glowings of heat were perceived in the intervals between the rigors or successions. These glowings grew gradually stronger, and continuing for a greater

length of time, at least banished every sensation of cold. The heat, which now succeeded, was often much above the temperature of health; marks of fever sometimes ran high; the veins became full; the face was flushed, and the surface of the body bore marks of distension. The duration of this state was uncertain: sometimes it did not continue the space of one hour, sometimes it lasted four or five. A dampness at first began to appear on the forehead and breast, which extending itself gradually to the extremities, was at least formed into a sweat; in consequence of which, the fever gradually subsided, and the body returned nearly to its natural state.

The above are the most usual symptoms of the paroxysms of an intermitting fever. I have described them in the order of time in which they most usually appear. I must however remark, that symptoms are sometimes observed different from those which I have now taken notice of; as also, that the order of succession, which I have mentioned, is not, by any means fixed and invariable. It is impossible to deny the common observation, that languor or debility is a general and early symptom in almost every species of fever; but it is likewise certain, that there are many instances, where it is not in our power to perceive its actual presence. It is therefore precipitate to conclude with Dr. Cullen, that all the future phenomena depend upon this, as their essential and original cause. There may frequently be deception in attempts to describe the situation of others; but that which we feel ourselves is more to be trusted to:—and I can affirm, that I have often felt sensations of cold in my own person, previous to every feeling of languor or debility; previous, I might even sometimes say, to any perceptible deviation from a state of health. But besides, that the existence or perception of languor and debility, does not seem to be essential to the existence of a paroxysm of intermitting

fever, I may likewise add, that I have seen instances, particularly in the hot months of summer, where the whole of this disease passed over, without the least perceptible degree of a previous cold fit. It happened sometimes also, that during the continuance of the paroxysm, there was scarcely any observable disorder in the pulse, or any material signs of external fever. The tumult and uneasiness, which terminate in most cases by sweat, went off in some by urine or stool, or perhaps declined in others, without the appearance of any preternatural evacuation. In like manner it was commonly observed, in the disease distinguished by the name of partial intermittent, that there was not any perception of cold, nor increase of heat; no disorder in the pulse, or preternatural evacuation; in short, not a symptom, which characterizes the genius of the disease, except local pain, which continuing for a certain time, disappears, and then returns again at a stated hour. To this we may add, that there are various instances, where the whole duration of a complaint, which indisputably depends on the cause of intermitting fever, is occupied by a comatose disposition, by convulsions, or even by tetanic affection. If we therefore consider these phænomena attentively, we shall find little cause to believe, that the most usual symptoms of the intermitting fever, are symptoms without which the disease cannot exist; or that they are mutually the cause and effect of each other. The order of succession I have observed is not fixed invariably; and cases are numerous, where those symptoms, which some authors have considered as absolutely essential, do not appear at all. This fact is certain; and we may safely conclude from it, that the main hinge of action in a paroxysm of fever has not been yet discovered.

The vital and natural functions are variously affected, not only in different people, but in the same person, in the different stages of a paroxysm of the

same fever. The pulse, in the first approach, is often remarked to be slower than natural, sometimes it is more languid and weak. It soon however becomes more frequent, though it continues for the most part small and contracted, till the latter stage of the cold fit. It then usually acquires strength and some degree of fulness, sometimes greater frequency and hardness; but as the sweat begins to flow, the hardness and frequency abate, while the fulness increases; so that it returns by degrees nearly to its natural state. The disagreeable affection of stomach, (which I formerly observed was sometimes the first perceptible symptom of a paroxysm of the intermitting fever) increases frequently to nausea or retching, sometimes to severe and continual vomiting; which does not cease till sweating has become general all over the body. The respiration, which in the beginning of the paroxysm, is usually slow, and sometimes interrupted with sighing, in the progress of the hot fit becomes frequent, laborious and high. It often happens, likewise, that there is more than ordinary dullness of perception in the mental faculties in the first approach of the fever; while this is often succeeded by extraordinary acuteness in the more advanced stages, particularly during the continuance of the hot fit. But though it is only during this period that excitement and delirium are observed to be common; yet instances are not wanting, where derangement of intellect is among the first symptoms of the disease, and where it continues among the principal throughout the whole of the course. To the above appearances we may add, that the urine, which is thin and pale in the first stage, becomes high coloured in the progress of the hot fit; and as the sweating advances, thick and turbid, frequently with the addition of a copious lateritious sediment.

I remarked formerly, in treating of the remitting fever of Jamaica, that certain hours of attack were

in a very peculiar manner connected with the different types or forms of that disease; but I cannot pretend to say, that the same rules were observed to hold good, with any degree of certainty in the intermitting fever of America. Single tertians, indeed, began most usually about twelve; though there were likewise many instances where they came on so early as ten in the morning, or so late as two in the afternoon. The other forms were still less regular. It was also taken notice of, that anticipations were common in the single tertian of Jamaica; as also that they were irregular and long. In America they were still more frequent; but seldom exceeded one or two hours at once. They often, however, prevailed to a certain acme, or point in the disease, observing a regular interval of time in their progress. It sometimes likewise happened, that the type postponed gradually, till the complaint disappeared finally. This, however, was much more rare than the other.

Having mentioned, in the preceding pages, some general resemblances of the intermitting fever of America, I shall next trace its peculiarities in the different provinces, in which the regiment to which I belonged, had the fortune to serve. I may observe, in the first place, that I joined the first battalion of the 71st regiment, on York Island, in the beginning of the summer 1778. Few of the men were then sick; neither did the number increase materially, till towards the latter end of June. The intermittents, which appeared previous to this period, were generally single tertian; and of perfectly easy treatment. In the month of July, a dysentery, of a very particular kind, became epidemic, and the sporadic intermittent instantly vanished. The stools in this complaint were numerous and bloody, the gripings were severe, but there was seldom any very material disorder in the pulse. The disease did not often terminate in less than seven days; sometimes it continued a

fortnight or longer. The ordinary treatment was very rarely of benefit; yet the complaint was of a nature so little dangerous, that I do not recollect a single person who died of it. It disappeared totally about the beginning of August, or rather changed into an epidemic intermittent, the type of which was usually single tertian. The paroxysms of this fever were regular, the intermissions were distinct; and its nature was so far from being obstinate, that I scarcely met with an instance which resisted the Peruvian bark, where that remedy was given in sufficient quantity. This fever continued highly epidemic during the months of August and September. The frequency of new attacks was considerably diminished in the month of October; yet such as happened then, were generally accompanied with dangerous and alarming symptoms. Relapses were common. But though the intermitting fever of this island was epidemic in a considerable degree, it was not by any means of a fatal nature. If neglected in the beginning, foundation was sometimes laid for obstinate complaints; but the disease was not fatal in its proper form to any one patient, who remained with the regiment. I cannot speak with certainty of the issue of a few of the worst cases, which were sent to the General Hospital, on the breaking up of the encampment in the month of November. The regiment was then embarked in transports, on an expedition to the southward. The sick were collected into one ship, which, after a stormy and tedious passage, arrived with the rest of the fleet at Savanna, in Georgia, in the latter end of December. The voyage had an excellent effect on the health of the men. Out of a hundred and twenty convalescents, who embarked at New York, in the month of November, not a man died; and there only remained two, who were unfit for the service of the field, on the day of our arrival in the Savanna river. During the months of January,

February and March, the battalion of the regiment in which I served, was a total stranger to sickness. It was employed in long and almost continual marching, till the latter end of April, when, encamping at Ebenezer, on the Savanna river, the intermitting fever soon made its appearance, and spread so rapidly, that before the end of June, very few remained, not only in this regiment, but even in the garrison, who had not suffered more or less from this raging disease. It was commonly remarked in the history of this fever, that the type during the month of May, was usually single tertian, till the fifth or sixth day; after which, paroxysms were often observed daily, though generally unequal in force and duration: that is, the disease changed about this period, to a double tertian form. But though this was observed to be the case, during the greatest part of May, the type of the fever was usually double tertian, or quotidian, from its very commencement, in the month of June. The disease was then of the most ardent kind. The paroxysms were seldom ushered in by a cold fit; and the remissions, for the most part, were very indistinct and imperfect. The heat of the weather was excessive, during the greatest part of the month; and strange and alarming symptoms occurred frequently in the course of the disease. In some cases a comatose disposition, approaching to apoplexy, or rigid spasms, resembling a perfect tetanus, occupied the greatest part of the paroxysm; in others there were various local pains, deliria, bilious vomitings or purgings, with a multitude of other affections, which appeared on a superficial view to constitute the whole of the complaint. Yet these symptoms declining after some continuance, recurred again at a stated hour, and were finally removed, or at least suspended, by the Peruvian bark. I left the garrison of Ebenezer in the beginning of July, and went directly to Savanna, where the same epidemic prevailed, though in a de-

gree of less frequency, and with symptoms of a less alarming nature than at the above-mentioned place. At Savanna, it usually retained marks of distinct intermission, and its type was often of the single tertian kind—in short, it was similar to the fever of Ebenezer in the month of May. From Savanna, I went to Beaufort in the beginning of August. The fever, which usually prevails at this season of the year, in all the southern provinces of North America, was then epidemic among the troops who were stationed on this island. The type, however, was still more commonly single tertian here, than at Savanna. The beginning of the paroxysms was likewise more generally distinguished by a cold fit; and the intermissions, for the most part, were more perfect and distinct. In a few cases, indeed, marks of malignity were discoverable; yet the disease, upon the whole, was not of a fatal nature, or of obstinate cure; though unless speedily checked by bark, it often degenerated into dysentery or dropsy, which were not only removed with difficulty, but in the circumstances under which we laboured, were often of very precarious issue. This epidemic was still acquiring force, when the outposts were summoned to the defence of Savanna. Its progress was, in some measure, suspended during the active service of the siege. The enemy, however, had no sooner retired from before the place, than a fever began to rage with violence, which carried off prodigious numbers, particularly of the foreign troops. It was observed in the history of the preceding year, that few were attacked afresh with the intermitting fever on York Island, so late as the months of October and November; but it was likewise remarked, that, where the disease happened at those periods, the symptoms were oftener malignant or dangerous. The same was in some respects the case at Savanna. The fever, which made its appearance after the siege, was of an alarming and violent kind. Marks of distinct

intermission were seldom discoverable, delirium was a common symptom, spasmodic affections were sometimes violent, and the course of the disorder was generally rapid. The rage of this epidemic ceased in December; but relapses continued to return occasionally, during the following winter; which was an unusually severe one in that southern latitude.

There likewise still remained some dysenteric complaints, which resisted every mode of treatment that could be devised. They yielded however to the return of the warm weather, assisted, in no small degree perhaps, by the active service of the siege of Charlestown. The recovery, indeed, was so complete, that, in the beginning of June, the whole of the regiment arrived at Camden in perfect health. The first battalion was sent to occupy a post at the Cheraws, on the river Pedee. The distance is seventy-five miles; yet such was the spirit and activity of the men, that they performed the march in three days, without fatigue or inconvenience. An open field, between four and five hundred paces from the bank of the river, was chosen for the encampment of this battalion; while a situation perfectly dry and cleared of wood, but nearer to the bank, was reserved for the encampment of the second, which was not expected to arrive till after some time. In a fortnight or three weeks, the intermitting fever began to shew itself. It spread so rapidly, particularly in the second battalion, that before the end of July, when the post was abandoned, few were left who had not felt its influence. The prevailing symptoms of this disease were much similar to those of the fever of Ebenezer. The type was frequently double tertian, or quotidian; the remissions were indistinct; the bilious vomitings and purgings were often excessive, and marks of malignity appeared in several instances. The approach of the enemy made it necessary that the post should be withdrawn; but there was much

difficulty in accomplishing it. Two thirds of both officers and men were unable to march; and it was not possible, in the situation in which we were placed, to find waggons sufficient to carry them, together with the necessary provisions and baggage; so that no other resource was left, than to convey some part of them to George Town by water. Boats were therefore collected for this purpose, and such men were put into them, as were judged least likely to be soon fit for the service of the field. These, however, unfortunately fell into the hands of the militia, in their passage down the river, and were soon dispersed into the different parts of the country; so that I cannot speak with certainty of the general issue of the disease. Those who retired to Camden by land, improved unexpectedly in the state of their health, in the course of the march. During the time that we lay at the Cheraws, the remissions were generally obscure; but after the second or third day's march, the type changed frequently from double to single tertian; at the same time that intermissions became clear and distinct. It may be difficult to determine precisely to what cause this might be owing; whether to removal from a situation, where the fomes of the disease was in a very concentrated state; to the mere exercise of travelling; or to the effects of cooler weather with rain, which happened at this time, and which continued for two or three days with little intermission. The whole of those causes, perhaps, contributed to operate this salutary change; though it will probably be reckoned among the first instances, where travelling and getting wet, are recommended as being useful in the cure of fevers. During the month of August, and a great part of September, the army remained encamped near Camden. The weather was excessively hot, and fevers were frequent,—sometimes malignant and dangerous; though they preserved, in general, the distinct character •

intermittents. In the months of October and November relapses were numerous, and original attacks, though rare, were dangerous and alarming when they happened. Some instances of a disease were now observed of a more serious nature than any that had hitherto appeared. Instead of distinct intermissions, which prevailed during the preceding months, the smallest traces of remission were scarcely perceptible; the countenance was dusky, and of a greasy appearance, the tongue was constantly dry and parched; the head was often much affected, and gangrenous spots sometimes appeared on the extremities. The duration of this disease often did not exceed seven days; sometimes it continued a fortnight, or even longer. It was generally of a fatal nature; and where it happened to people who had been subject to the intermitting fever in the preceding months, it for the most part effected such a change on the constitution, as destroyed the tendency to relapse. But besides this unusual species of disease, which sometimes appeared in the months of October and November, it was likewise observed that relapses of the fever, which preserved the distinct intermitting character, were not only less frequent, but commonly less alarming, in proportion as the weather turned cooler. Relapses were often remarked in this season to terminate of their own accord, in a very short time; and frequently to leave the body in a state of greater vigour than they found it. I find a fact in my notes, with regard to this subject, which is curious and important. Between thirty and forty of the men of the regiment entered upon the service of the campaign in so weak a state, that they were unable at first to carry their arms. They however gained strength speedily as they proceeded on the march; and seldom forgot to mention, that they felt a new accession of vigour after every accidental relapse. But I must further observe, that, together with the

above changes which happened in the progress of the season, the epidemic shewed a remarkable tendency to degenerate into dysentery or dropfy in the months of September and October. The gripings in this species of dysentery were often severe; the stools were large and watery; and times of aggravation and remission were frequently observed, as in a regular intermittent. Indeed the intermittent, the dysentery, and even the dropfical swellings so often alternated with one another, as evidently shewed that they all depended upon the same general cause. The campaign of the following winter was a very active one. The army travelled over a great extent of country, and was considered by many as performing very hard service; but I have the satisfaction to add, that notwithstanding occasional forced marches, wading of rivers, exposure to rain, accidental scarcity of bread, and no great profusion of beef, with the total want of rum, the troops enjoyed in general a most perfect state of health. Valetudinarians were restored to perfect vigour; and when we arrived at Wilmington, in the latter end of April, there scarcely was a man in the regiment to which I belonged, who was not fit for the duty of the field. In the summer campaign through North Carolina and Virginia, there was no room to complain of hardships. The camp abounded with a profusion of the best provisions; and the marches were seldom long or fatiguing. We arrived at Portsmouth towards the end of July, with a very moderate list of sick. Portsmouth is said to be unhealthy; and we soon were able to verify the observation: an intermitting fever, complicated, or alternating with a dysenteric complaint, made its appearance soon after our arrival, and continued to increase during the short time we remained in the place. A disease of a similar kind continued to prevail in the army, after our removal to York Town. It was not, however, by any means fatal in its nature,

or difficult of cure, if attended to in time, though if allowed to go on, it often degenerated into dropfy, obstructions in the abdominal viscera, or a dysenteric complaint which frequently proved fatal in the beginning of the following winter. The 71st regiment had now served three campaigns in the southern provinces, and might be considered as being perfectly well seasoned to the climate. It was in fact more healthy than any other corps in the army; there not being more than five or six unfit for the duty of the line, when the French and Americans invested the place. After the capitulation the proportion of the sick of the army increased considerably. Some instances of a fever, similar to that which prevailed at Camden and Savanna, in the month of November, were observed in several regiments; but a species of dysentery, which appeared often to have originated from an ill cured intermittent, was the complaint which proved principally fatal.

From the above short history of the intermitting fever, as it appeared in the 71st regiment, in the different provinces of North America, where that corps happened to serve, we may be enabled to form some idea of the changes, which are more constantly produced by season and climate, or which arise accidentally from the particular effects of local situation. In the spring and beginning of summer, the single tertian was the most usual type of the endemic of America, in every province which the regiment visited:—the paroxysms were distinct, and the intermissions were generally perfect. In the months of June, July and August, double tertians were common, and in some situations banished every simpler form. As the weather turned cool, the single tertian resumed its place; so that any other type was scarcely ever seen. But besides the above changes of type, which in some degree followed the changes of season, dysentery or dropfy frequently made their appearance in the months

of August, September and October, alternating with, or succeeding the intermittent; while fevers of a bad and uncommon kind were by no means rare in the months of October and November.

I have thus observed in a cursory manner the more general changes of the intermitting fever, as influenced by change of season. I may also remark, that besides season, climate had a considerable effect in modifying the appearances of the disease. It thus happened, that the type was generally single tertian on York Island, even in the heat of summer; in spring and winter other forms were rarely seen. In Georgia, the single tertian was the prevailing form, only in the winter and spring. In summer, and some part of autumn, double tertians were common; and types of still greater complication frequently made their appearance during this period, in some particular situations of the province. Dysentery dropsy and dangerous fevers were likewise more frequent here in the autumnal months, than they were found to be in the neighbourhood of New York; while the course of intermittent, as long as the form was regular, was more speedily checked by Peruvian Bark in Georgia, than in the more northern latitudes. The prevailing type of the climate of South Carolina, was single tertian, even in the summer and autumn; yet where the forms of the disease was in a high state of concentration, as at the Cheraws, the type was often so complicated that remissions were scarcely discernible. The tendency of the endemic of this province, to degenerate into dysentery or dropsy in the autumn, was likewise less remarkable than in Georgia. The dangerous fevers of October and November were also fewer in number; though still more frequent, and more formidable than in the province of New York. The province of Virginia lies about halfway between New York and Savanna; and the general effects of its climate, on the common endemic of the country, cor-

responded with its local situation.—Deviations from the tertian type were more frequent than at the one place, less so than at the other.

It appears from what has been said above, that the single tertian is the proper fundamental type of North America. It undergoes, as we have seen, a regular change and alteration, in consequence of the ordinary changes of the seasons, as also in consequence of the effects of the various climates of the different provinces of that extensive continent: but besides these changes, which are more general and certain, we likewise find, that the accidental circumstances of local situation often produce very remarkable effects. In this manner, though the type of the fever which prevailed on York Island, was properly single tertian; yet double tertians, and even more complicated forms, were not by any means rare, in a part of the battalion which lay contiguous to a swamp. The real nature of the endemic fever of Georgia, is, perhaps, properly of the intermitting kind; yet remissions were often scarcely perceptible at Ebenezer; which is situated immediately on the bank of the river Savanna, and which, in some degree, is surrounded by creeks of fresh water. It may not be improper to remark with regard to Ebenezer, that few places in America have been observed to be more unhealthful; though such a conclusion probably would not be drawn from a general view of its situation. It occupies a sandy eminence of considerable elevation, and possesses a considerable environ of cleared ground. At Savanna, which is situated twenty-five miles nearer the mouth of the river, there were likewise many instances of deviation from the single tertian type, but still fewer than at Ebenezer. The fever likewise was generally of a less dangerous kind. The obvious appearances of the two places did not afford sufficient reason for forming this conclusion. The situation of Savanna would have probably been thought

to be the least favourable to health. Though elevated and dry, and possessing a wider environ of cleared ground than Ebenezer; yet a swamp on the right and left, with a river and rice swamps in front, threatened great ravage from intermittents. That they were less formidable than might have been expected, was probably in a greater measure owing to the bluff or sand bank being higher than the situation of the town, and intercepting, in some degree, the exhalations from the river and great swamps.

I observed on a former occasion, that the signs of crisis, in the remitting fever of Jamaica, were generally clear and unequivocal. I must now own, that I have not been able to attain certainty, on this head, in the intermitting fever of America. In fevers of a single tertian type, the intermissions were frequently so perfect and complete, that it was not easy to say what was wanting to constitute perfect health: even in the hot months of summer, where the remissions were extremely obscure, I often found it difficult to form an opinion to which I could confidently trust; as it happened frequently, that those signs, which I had been disposed to consider at one time as marks of final crisis, proved in the event only to be indications of more distinct intermission, or of some change in the nature of the symptoms.

Having given a short view of the history and progress of the intermitting fever of America in the preceding pages, I shall now proceed to offer a few observations on the manner of treatment. And I may observe in the first place, that the intermitting fever is not in general a disease of a dangerous nature. If treated with decision in the beginning, it is for the most part removed very speedily and very certainly; though if attacked with feeble remedies, it often continues long, and not seldom lays the foundation of complaints which eventually have an unfavourable termination. The intermitting fever sometimes proves

fatal from the actual violence of the symptoms of the paroxysm, though the danger more generally arises from a tendency to degenerate into dysentery or drop-
sy, or to form visceral obstructions. But besides the danger, which arises from the actual force, or from the more tedious effects of the disease, we often find a character of peculiar malignity, in the intermittents of some seasons and some situations, which deserves to be particularly attended to. Malignity is a word of a vague meaning; and on different occasions is differently applied. In the present instance, I refer the term to a peculiar character of the disease, expressed by a certain state of the eye and countenance of the patient. It was sometimes observed, that the countenance of the patient was flushed; but at the same time dark and overcast; or that it was of a greasy and dusky appearance, with a look of sternness and despondence in the eye. Those appearances, particularly where a white glutinous covering appeared on the tongue, were constantly indications of concealed or lurking danger. They occurred often at Ebenezer and the Cheraws, and I had the misfortune to learn from experience, that whenever they occurred, time ought not to be spent in the frivolous preparations, which are usually thought necessary to precede the giving of bark. If we judge it not to be proper in any case to venture upon the use of bark, before the body has been sufficiently prepared by emetics and cathartics, we shall too often meet with instances where the course of the disease will be finished before these preparations are completed. It has happened oftener than once to myself, that the fatal paroxysm commenced before I had gone through the usual routine of preparation, which at that time I thought indispensably necessary, before I attempted to cut short the course of the fever by means of its well known specific.

Having premised so much with regard to the ge-

neral nature of the disease, I shall now add a few observations about the management of those remedies which have been most usually employed; some of which appear to be superfluous; while there is only one of them which has a right to be considered as effectual. It is a common and obvious remark, that the intermitting fevers of the same season shew a general tendency to run over a similar course, though the modes of treatment may be sometimes directly opposite. As I had often taken notice of this fact during the time that I remained in America, I began to suspect that the changes, which I had been accustomed to attribute to treatment, were in reality owing to a disposition in the nature of the disease, which was little affected by the ordinary prescriptions. But that I might in some degree ascertain the truth of this suspicion, I selected thirty cases of fever, which had commenced within twenty-four hours of each other. This experiment was made at Ebenezer, in the month of May; where the disease, though highly epidemic, had not yet discovered any signs of malignity; so that danger was not apprehended from a delay of a few days. I classed those thirty cases in three divisions, without paying regard to the nature of the symptoms of any individual case. To one I prescribed a repetition of emetics, at such intervals as were judged proper; for another, cathartics of various kinds, or managed in various manners; while I left the third wholly to its own course. I watched the progress of the disease attentively for the space of eight days, and cannot pretend to say, that I observed any material difference in the changes or appearance of those cases, which were treated in so different a manner. The type, which was generally single tertian at the commencement of the disorder, changed for the most part to double tertian or quotidian, after the second or third paroxysm; though not in a different proportion, as far as I could judge, in those which were left

entirely to Nature, or which were treated in the manner which has been mentioned above. It deserves however to be remarked, that the paroxysms were usually milder, more regular and distinct after the repeated use of carthartics; as also that bark succeeded more speedily and more certainly after a repetition of antimonial emetics.

Where bleeding is judged to be proper or necessary in the cure of the intermitting fever, the circumstances are generally such as require that it precede every other in order of time. Bleeding was often found to be useful in particular cases. It moderated the violence of symptoms, and seemed not to be without effect in removing a certain state of the system, which resisted the successful operation of the bark; but I must likewise add, that there was seldom occasion to employ it in the southern provinces of America. It was blamed by some as increasing the disposition to relapse: of this, however, I can say nothing from my own experience; and as I am disposed to believe, that the loss of a moderate quantity of blood, seldom does material harm in this disease; so I have reason to think, that it is seldom necessary in warm climates, particularly in the hot months of summer or autumn.

Emetics have been employed in the cure of intermittents for a long time past. There are many practitioners, who consider their use as indispensable; and some have pretended, that the cure of the disease, in its earlier stages, may be trusted to this remedy alone. Emetic tartar, so managed as to operate at the hour of the fever's return, was sometimes found to prevent the access of a particular paroxysm; but though obliged, from want of bark, to have recourse to this method of treatment in numberless instances; my experience does not supply me with a single one, where I could positively say, that it absolutely cut short the course of the disease. Relapses, I must

confess, disappeared frequently in consequence of the practice; but they likewise disappeared frequently, where nothing at all was done: so that I cannot help being of opinion, either that the real effects of emetics have been mistaken, or that the proper modes of managing them are not generally known. I must not however omit to mention, that emetics are occasionally of great service, and that antimonial emetics especially obviate the effects of inflammatory diathesis, and on particular occasions facilitate the successful operation of the bark. They are likewise seldom followed by those dangerous effects which frequently arise from the employment of them in fevers of Jamaica; though I certainly should advise that they be used with great caution in the southern provinces, in the summer and autumn, where the remissions of the fever are obscure.

Cathartics have likewise been much employed by practitioners in the cure of intermitting fevers; and in few cases, perhaps, can be omitted with safety. They certainly possess very remarkable effects in rendering the form of the disease regular and distinct; but they do not seem to be endued with a particular power of cutting short its course.—The circumstances of the case can only point out the propriety or advantage of the various remedies of this class.

The above are the usual modes of evacuation, which most practitioners consider to be indisputably necessary previous to the exhibition of bark. I must however remark, that cases sometimes occurred in the southern provinces, particularly in the hot months of summer, which only admitted of those evacuations in a small degree. Instead of the distinct and regular paroxysms, which were expected to follow the use of emetics or cathartics, the disease was sometimes observed to assume a languid and continued form, in consequence of such treatment, while it likewise in some instances recovered the distinction of paroxysm

and remission, by means of applications, which excited the tone and vigour of the system; particularly by means of exercise in cooler air, or accidentally by exposure to rain.

Peruvian Bark is the chief remedy, upon which we now depend, for the cure of intermitting fevers. It is a remedy, which like others, has undergone some reverses of reputation, since its first introduction into Europe; and, though its efficacy is now fully acknowledged, perhaps over-rated by English practitioners, it does not seem yet to have gained the same general credit with other European nations. The French use it with caution, and many of the Germans are still its enemies. It has been accused even by some of the English writers, of failing in the cure of intermittents; and blamed by many of occasioning complaints more dangerous in their nature than those it was intended to remove. I was early aware of these objections, and watched narrowly that I might discover its real effects; and am warranted in saying, that it has every right to be considered as a specific in ague and fever; while it is totally free from the imputation of occasioning dysentery, dropsy, or visceral obstruction. Those complaints were always most frequent, where this remedy had been the most sparingly employed.

But though I have mentioned that bark is both a safe and efficacious remedy in the cure of intermitting fevers, I must also confess, that it is only rendered so by particular modes of management. It is probable that much of the bark, which is now imported into England, is either in some shape adulterated, or naturally inferior in quality to what it had been in former times; as we often read of cures effected by a single drachm in the last century, which we should scarcely now expect from an ounce. Three or four ounces seldom failed of checking the progress of the most formidable fevers of America; one or

two frequently did not produce any sensible effect. Being perfectly convinced of the truth of this observation, I generally gave bark in cases, where the circumstances were judged proper for its exhibition, in doses of two drachms; which I directed to be repeated every two hours during the absence of the fever. By this mode of treatment the disease was often so completely conquered, that the patient was frequently capable of returning to his duty in the space of seven or eight days. Time ought not to be spent in frivolous preparations, or diseases attacked with feeble remedies, where the health of soldiers is concerned. The speediest cures are generally the best; and I have had many opportunities of witnessing more health and strength gained during eight days in the field, than I should have expected from a month's ease in an hospital, assisted by the best advice of the physicians. The above-mentioned quantity of bark, for the most part, was sufficient to effect a cure in ordinary cases of the disease; but where danger appeared to be threatening, the doses were often increased to half an ounce, or even more. In some cases of obstinacy, indeed, accompanied with a sluggishness of constitution, I observed no other rule in the quantity than such as proved disagreeable to the stomach, or excited a tumult in the system. The method was often successful; and I may observe in general, that two ounces taken at five or six times, and in the space of eight or ten hours, were often more effectual, than double the quantity in small doses, and at long intervals. If the large doses of bark, which I have recommended, should appear to any one to be dangerous or unnecessary; I may add, that I have myself frequently taken an ounce at once; while I have likewise observed the cure to be tedious and uncertain with the moderate doses of ordinary practice. I may farther remark, that this remedy was often rejected by the stomach, and in some cases

passed off almost instantly by stool ; yet that the course of the fever seemed to be no less effectually checked by it, than when such effects did not occur.

The quantity of bark, which I frequently prescribed in the intermitting fever of America, may appear to be greater than necessary : so the time at which it was sometimes given, may also appear to be premature. Bark was seldom given in the fevers of the spring and beginning of summer, unless in cases of relapse, till after sufficient evacuations had been premised. In the autumnal months, where signs of malignity and danger were discovered, the first intermission was often laid hold of : neither was it uniformly deemed necessary, to premise the evacuations of vomiting or purging. In relapses I seldom let pass the opportunity which the first intermission afforded ; by which means, those who were subject to the returns of this disease, were rarely returned in the reports of the sick.

Where bark was given in such quantity, and in such manner as I have mentioned above, it generally was successful in checking the progress of the disease ; yet I must not omit to observe, that instances sometimes occurred, where it totally failed of this effect, under every mode of management that could be devised. It would be useful to ascertain those circumstances exactly ; but this is a task which I cannot promise to perform. It was however remarked, that where the inflammatory diathesis prevailed in very evident degree, bark was frequently given without success : and owing to this cause, perhaps, it was less to be trusted to in the spring, (unless in cases of relapse), than in the hot weather of summer and autumn. But besides the obvious marks of inflammatory diathesis, there appeared to be other conditions of the frame unfriendly to the successful operations of bark. The exact nature of these, indeed, was perceived with difficulty ; yet I have often observed

them to be connected with some of the following circumstances; viz. with a small and hard pulse, or with a pulse where the stroke was obscure or without expansion, and where a creeping or vermicular motion was observed in the state of the artery; to which was sometimes added, a constricted state of the skin, a clammy moisture on the surface of the body, without signs of free perspiration, and together with a suspension or irregular action of the nervous influence. In the state described above, bark alone was often given in great quantity, without producing any sensible effect. It sometimes succeeded where antimonials, opiates and other antispasmodics were joined with it; but blisters applied to the back part of the head and neck, were serviceable above all other remedies in removing those circumstances, whatever they were, which stood in the way of its successful operation. In some cases which had proved obstinate to every other means, the disease disappeared immediately after their application; and in every one where they were employed, ceased any longer to resist the bark. Bark alone undoubtedly has a right to be considered as a specific in the cure of the intermitting fever, but its virtues are occasionally improved by the addition of aromatics, chalybeates, and particularly by a certain proportion of snake root. It was a practice with some of the country people of Carolina, to attempt the cure of the intermitting fever by means of Virginia snake root, given in doses of two scruples or half a drachm. I made a trial of that remedy in several instances; but did not find that it was successful. Joined however with the Peruvian bark, in the proportion of two drachms to an ounce, it was often observed to produce very excellent effects. The cures were more complete and more permanent. There was not only less tendency to relapse, but dysenteries and dropries were more rare, after I was fortunate enough to adopt this mode

of practice, than they had been during the preceding years.

There have been many different conjectures about the mode of the bark's operation in the cure of intermitting fevers; but none of them afford a satisfactory explanation of the subject. Bark is evidently bitter, astringent and aromatic; but how it becomes specific in intermittents, in a degree so superior to all other bitters, astringents and aromatics, is a mystery we cannot easily comprehend. It obviously possesses a power of giving tone and vigour to the powers of life; and often communicates to the sanguiferous system, a certain state or disposition, known by the name of inflammatory diathesis. Thus it is usually observed, that where the nervous frame is weak and delicate, bark rarely failed of cutting short the course of the disease, and its success in such cases, is preceded, for the most part, by apparent changes in the general diathesis of the system. If strength, fulness, and vigour of pulse follow the employment of bark, the fever frequently disappears; but if those signs are wanting, it is not possible to form any certain judgment of the future effect. In like manner, if signs of inflammatory diathesis continue to prevail during the course of the disease, bark sometimes changes it to a continued fever; but seldom checks its progress effectually. From these facts, which I have often seen verified, I am disposed to conclude, that bark is only to be considered as an accidental specific in the cure of intermittents, and that its salutary effects probably may be explained from the change, which it occasions in the relative state of the body. That bark is so often successful depends, we may presume, on the intermitting fever being so often connected with an attonic state of the system; from which cause it probably arises, that it is so much more effectual in summer and autumn, in warm weather and in warm climates, than in the

opposite circumstances. But though an attonic state of the system is frequently connected with intermitting fever, it is not constantly so; hence the disease is not invariably removed by those processes which excite tone, or give rise to the inflammatory diathesis. Excessive evacuations and other causes, by which the body has been reduced to the last state of debility, have often interrupted the course of an intermittent. So that we have every reason to conclude, that bark is only relatively specific, in as much as it destroys certain circumstances of aptitude, which are essentially connected in particular situations with the existence of the disease. In this manner, voyages, journeys, new pursuits, or new modes of life, frights, the active service of the field, or the hardships of sieges have often removed intermitting fever, which had resisted the ordinary aids of medicine.

I observed in a former part of this treatise, that it was seldom a matter of much difficulty to stop the course of the intermitting fever of America; but I must now add, that it was always difficult, sometimes impossible to secure the patient against any future return of the disease. Bark, though much celebrated for this purpose, did not seem to prevent a relapse with the same certainty with which it stopped the course of the fever, when actually present. It deserves however to be remarked, that where bark had been given at regular intervals after the disease disappeared, the paroxysms in the relapse were, in general, not only slighter, but had usually more of the inflammatory diathesis joined with them, while the complaint shewed more disposition to terminate of its own accord, after a few revolutions. It is universally known, that the powers of bark seldom fail in the cure of intermitting fevers, where given in sufficient quantity; yet I must also observe, that its virtues do not seem to extend farther than to a temporary suspension of the paroxysms. That bark does not eliminate or

destroy the actual cause of the disease, appears plainly from this fact, that relapses are frequently the consequence of those circumstances, which occasion debility, or which counteract the effects of this tonic remedy. To which we may add, that though relapses are often of a different type from the original fever, yet, as they generally happen on an even day from the suppression of the paroxysm, there can be little room to doubt that the old complaint again resumes its course, though it probably, in the mean time, loses several of its original symptoms. It is a fact likewise which we ought not to omit mentioning, but which in general, does not seem to be much attended to, that some periods are more remarkable for the relapse of intermitting fevers than others. I observed before, that relapses almost constantly happen on the even days. I now add, that the most remarkable of these days are the sixth, the eighth, twelfth, fourteenth, twentieth, twenty-second, twenty-eighth, and thirtieth. The fourteenth is remarkable for relapses above all the others. Next to it we may rank the twelfth, twentieth, and twenty-second; unless in times of very prevailing sickness, where the sixth and eighth often come in for a great share. If we take pains to examine the particular circumstances of the patient, and attend to the nature and degree of the prevailing epidemic, we may often be enabled to form a tolerable conjecture with regard to the most probable period of return. Having therefore acquired from observation some general ideas of the different propensities to relapse in different situations, and in different subjects, I usually began to give the bark in quantity, and to use other precautions on the fifth, after the suppression of the paroxysm, in cases where there were the strongest suspicions of a speedy return; while this was delayed till the eleventh, nineteenth, or twenty-seventh in others, in proportion to the different degrees of healthiness. This practice was

continued for the space of three days, or till the suspicious period was past. But I must further remark, that besides the propensity, which was observed in fevers to return at the periods above-mentioned, the approach to the new and full moon was likewise found to be connected with relapses in a very remarkable manner. Independent therefore, of the precautions, which were used at the septenary periods, the approach to new and full moon was constantly attended to.

If the above directions were carefully complied with, we might in general prevent the disease from proving fatal, or from injuring the constitution materially, though I must at the same time confess, that we could in reality do little more than preserve the patient in a valetudinary state, till cool weather, a change of situation, or such a change in the manner of life as excited the active powers of the constitution, contributed their part to effect a permanent establishment of health. Being perfectly convinced of the truth of this observation, I generally remitted the men to the regiment, to be put upon the list of duty, as soon as I was certain that the course of the disease was actually stopt. The practice at first sight may appear harsh; but I have found it to be salutary. Exercise, even some degree of exertion, promotes the recovery of health. Habits of sloth and indolence are speedily contracted in hospitals: the military ardour is gradually extinguished, and bodily strength is recruited more slowly than in the field, under every disadvantage of fatigue or inclement weather.—Of the truth of this I have had ample experience.

I have now pretty fully described the method of cure, which I pursued in the intermitting fever of America. I am not ignorant that other plans have been adopted, and other remedies employed by others; but as I have not had experience of any, except that which I have mentioned, I do not reckon myself

qualified to furnish any remarks on the subject. Bark, indeed, is so safe, and at the same time so effectual, that I should be slow in recommending any other remedy, where this can be procured in sufficient quantity. The strongest proofs of its value arise from a comparative view of the mortality of the intermitting fever, in different regiments, which were employed on the same service, but which were treated in different manners by their respective surgeons.—The Hessians were all of them inveterate enemies to the bark; and there were ever some of the British surgeons who employed it very sparingly. The mortality among the troops trusted to the care of those, was uniformly in great proportion. There was a Hessian regiment, the situation of which I had the opportunity of knowing exactly, that lost one third of its men by this disease and its effects, during one year's service in Georgia. There were British regiments also, which lost more than a fourth; while there were others, which did not lose a twentieth. The whole of these regiments were engaged on the same services; they were all alike foreigners in America; and there appeared to be no obvious cause for so great a difference in the degree of mortality, except a difference in the management of the bark. Bark was scarcely ever employed in one case; in another it was used with timidity; whilst it was given with the earliest opportunity, and in quantities far exceeding the usual practice in the third.

I have described, in the preceeding pages, the method which I adopted in the cure of the intermitting fever of America, whilst that fever preserved its distinct and proper form. I have likewise mentioned the best means I am acquainted with, of guarding against its return; and it will not be improper in the next place, that I add a few remarks on some of its most usual and formidable effects. The intermitting fever of America shewed a strong disposition to

change into a species of dysentery, or a purging and griping at particular seasons of the year, and more especially in particular local situations. Dropsy was likewise a frequent effect of this complaint, and obstructions of the viscera were not by any means uncommon, where the intermittent, from neglect or other circumstances, had been allowed to go on in an uninterrupted course.

The changes from intermitting fever to dysentery, and from dysentery to intermitting fever, were so frequent in the months of August and September, that those diseases seemed evidently to depend on the same general course; assuming at different times the one or the other form from causes which we could seldom ascertain. In those cases of dysentery the stools were uncommonly copious and watery, and remissions and exacerbations frequently appeared at regular periods; but though the cure was often attempted by the bark, it did not in general succeed.

I must begin with acknowledging, that I shall not be able to give a complete or accurate history of the progress and final termination of this species of dysentery, into which the intermitting fever is so much disposed to degenerate; as I shall likewise only have it in my power to mention the general methods of cure, which were pursued in the earlier stages of the disease. Where it was found that a cure could not be accomplished in a reasonable time in the field or regimental hospital, dysenteric patients were generally removed to places where they could meet with better accommodation; so that the disease in its latter stages has seldom fallen under my observation.

I must observe in the first place, that this species of dysentery had no right to be considered as an infectious disease. It appeared in fact to be no more than an intermitting fever, which, from some cause or other excited its principal force on the alimentary canal. Remissions and exacerbations were generally

observable in the one disease, as well as in the other in the earlier stages; yet these appearances became gradually less and less remarkable, and there appeared at last marks of permanent affection of the intestines. The skin now became dry and harsh, the flesh wasted, and the bowels were uncommonly irritable, particularly where the *prickly heat* had retired from the surface; the disease was now evidently supported by the existence of permanent local affection.

With regard to the cure of this species of complaint I have little to observe, which is not generally known. Bark was sometimes employed to check its course; yet I must confess, that bark alone was seldom found to be successful. Where there were no marks of an actual inflammatory state of the stomach and bowels, it succeeded better when joined with aromatics, powder of camomile flowers, and particularly with snake root. Laxatives were prescribed frequently, and seemed often to be proper. They were however more useful where some other thing was joined with them, which had the quality of determining to the skin. Opium in various forms was a common remedy; and often a useful one before there were marks of permanent affection of the intestines. Ipecacuanha, or such preparations of antimony as promoted the evacuations by the skin, were frequently combined with it. The great object which I pursued in this disease was to restore and support a free perspiration, to diminish the irritability, and to strengthen the tone of the alimentary canal. I was disposed to expect benefit from warm bathing, frictions, &c. but the situation in which we were placed did not admit of a trial of them. Exercise was proper, and even some degree of exertion. Change of air was serviceable in many cases, well seasoned food in some, and wine in others. The above plan was pursued in the beginning of the complaint with tolerable success; but if it failed, or could not, from the

circumstances of the service, be properly executed, the disease then lost the remitting form, the body became lean and exhausted, the stools bloody, with a very irritable state of the bowels. In such cases there was often ulceration, various degrees of inflammation, or obstructions in the coats of the intestines. Blisters applied to abdomen or loins, and kept open, were often serviceable; medicated glysters, varied according to the nature and seat of the affection, were likewise of benefit; and in the latter stages I have seen much good from the employment of strong astringents. In illustration of this I shall mention the case of an officer, who was attacked with this species of dysentery, soon after the siege of Savanna. Every thing, which the medical people of the garrison could suggest was put into execution, without much benefit. The disease continued through the whole of the winter without material abatement; the flesh wasted, the skin became dry, with such other symptoms as are usual in this complaint. In the month of March, a person of the country recommended the decoction of the bark of a tree, (probably of the species of the Simarouba,) which appeared to be possessed of a considerable share of astringency. It checked the purging and griping instantly; so that the disease ceased for the space of three weeks. The appetite was good, the stools copious, and in some degree lienteric. The griping and even purging at last returned; the decoction was repeated, but had not the same effects. Other astringents, the extract of logwood, terra japonica, &c. checked it for a short time, but no material ground was gained. He died in the month of May.

Dropfy succeeding, and sometimes alternating with intermitting fever, was not by any means a rare appearance in America, particularly in some seasons, and in some situations. The swellings generally prevailed in every part of the body. They were usually

leucophlegmatic, though I have also seen some instances of tympanitis; a disease which was commonly supposed to proceed from improper management of the bark.

With regard to the cure of dropical complaints, I have little to observe which is not generally known. It consisted not only in evacuating the water, but in communicating to the system such a degree of tone and vigour as resisted farther accumulation. With this view exercise, and even some degree of exertion, was proper: wine, even more stimulating liquors, high seasoned food, frictions and warm clothing, were serviceable. I should likewise have been disposed to expect benefit from sea bathing; but I cannot venture to say, that I have ever made a proper trial of it. Blisters were also useful; not only as occasioning a discharge of the waters, but as exciting the action of the vascular system. Among the numerous class of diuretics, there is not any one, which has so powerful effects as cantharides, in substance or in tincture; and among the corroborants, I should be inclined to give the preference to chalybeates, colombo root, and Peruvian bark. It may not be improper to mention in this place, that I have seen some instances where a general anasarca has been completely cured by the accidental supervening of convulsions.

Obstructions of the abdominal viscera are likewise reckoned among the common effects of intermitting fevers. They are frequently attributed to the early or improper use of bark; but appear in fact generally to arise from the long continuance of the disease. I cannot add any thing to the manner of treating them that is not known to every one.

C H A P. XIII.

A GENERAL REVIEW OF THE PRACTICE OF PRECEDING AUTHORS IN FEBRILE DISEASES.

HAVING described the method of cure, which I followed in the remitting fever of Jamaica, the yellow fever and intermitting fever of America; it will not, I hope, be deemed superfluous to give a short sketch of the general principles, which have directed the practice of physicians in febrile diseases, from the days of Hippocrates, till the present times; a subject of which I have not yet seen a connected view. There is reason to believe that the science of medicine made considerable progress in different parts of the world, particularly in Egypt, at an early period; but distinct records of the art, prior to the age of Hippocrates, are either lost, or so blended with the writings which are assigned to that author, that we do not know how to distinguish them. The practice of Hippocrates in fevers, has been accused of being feeble and inert; and it is certain, that many of the most effectual remedies of modern times, were unknown to that venerable physician; yet if we take the pains to estimate candidly the whole mode of proceeding, we shall be forced to acknowledge, that its effects were less inactive than has been generally imagined. The mode of treatment adopted by the Coan Sage, seems not only to have done evident good, but sometimes to have actually cut short the course of the disease. Sweating at an early period, the most effectual means we yet know of cutting off the course of fevers, was employed frequently by this author; and though antimonials were then unknown, yet sweat, excited by a simpler process, was often observed to produce very decisive effects. Hippo-

crates, indeed, has been less circumstantial in the detail of remedies than we could have wished. He has however related the dietetic part very distinctly. His rules are always judicious, and his regimen sometimes of such efficacy, as evidently to operate very considerable changes in the state of the system. It appears frequently to have been his aim to attempt to exterminate the fever in its early periods, or to endeavour to cut short its course abruptly, by direct or indirect opposition to its proximate cause: yet this idea was not pursued beyond a certain point. After the fourth day had passed, Hippocrates usually contented himself with supporting the general powers of life with proper diet and nourishment, allowing nature after that period to perform the work her own way: in other words, to complete the business by the slower operations of coction and crisis. These two views, viz. the attempts to cut short the disease abruptly in its beginning; or in the late periods, the endeavours to support the powers of life, till the natural termination might arrive, comprehend the general rules of practice followed by this celebrated physician: and I much doubt if the moderns, notwithstanding all their pretensions, have actually discovered any other indications more decisive, though I willingly allow, that they have made great improvements in the mode of executing those I have mentioned. At least, I acknowledge for my own part, that I am not yet acquainted with any mode of treatment, by which the natural course of a continued, or even obscurely remitting fever can certainly be prevented, after the first days of the disease are past;—that is, after a distinct formation of the type; before that happened, the sweating process is frequently successful. But though the attempt to cut short the disease in its beginning, or to support the powers of life in the later periods, comprehend this author's general rule of practice; yet we find considerable

diversity in the manner of accomplishing these different purposes. The *ἐναντία ἐναντίας*, or an attempt to counteract the derangements of morbid causes, may be considered as the first general maxim, which was established in the cure of diseases. Hippocrates has this maxim constantly in his eye, and endeavours by various means, according to a supposed diversity in the mode of action, to cut off the immediate existence of fever. In this manner the presence of heat and bile, or the supposition of obstructed perspiration, have severally furnished him with different indications. His ideas however are not precise; so that his practice frequently fluctuates between conjecture and experiment. If the disease does not yield to one mode of treatment in a given time, he frequently passes to its opposite much at random.

The doctrines of Hippocrates acquired such general credit, in every part of the world where they were known, that we do not remark any material innovations in the cure of fevers, till the time of Erasistratus; a space of near two hundred years. The intervening period, indeed, was distinguished by philosophers, who employed their time in investigating the structure and economy of the human frame, as well as by physicians, who improved the art of medicine by the invention of new remedies. Plato, Aristotle and Theophrastus were the most eminent of the former; Petro is chiefly distinguished among the latter. (1) This author, as we find recorded both by Celsus and Galen, attempted to extinguish a fever by copious drenching with cold water; soon after which, he nourished his patient with wine and strong foods, a custom which was in some degree imitated by Clophantus. Hints, however, of the first of those practices are found in the writings of Hippocrates, so that we may justly consider Erasistratus, as the first who departed so far from the principles of the Coan Sage, or who arrived at so high reputation, as to be

regarded by posterity as the author of a new method of curing diseases. I mentioned in a former part of this treatise, the opinion which Erasistratus entertained concerning the cause of fevers. All that we know of his practice may be comprised in a few words. Erasistratus was every where the inveterate enemy of bleeding. He was likewise the author of a certain plan of abstinence, which, with a little modification from Asclepiades and Themison, made a conspicuous figure in the annals of physic for several succeeding ages.

Herophilus, who lived much about the same time with Erasistratus, acquired also high reputation in the medical world; but unfortunately we have it not in our power to give a particular detail of his discoveries. Soon after the period I have mentioned, the science of medicine was divided into three distinct branches; viz. as the art is employed to remove diseases by diet, by drugs, or by manual operation. Serapion likewise, who is considered as the author of the empiric sect, made his appearance not long after this division of the profession into separate branches. His followers were numerous, and many of them were respectable; but we are not enabled to give a distinct account of their practice on the subject of fevers. The summary views of Celsus and Pliny, or the accidental fragments in the voluminous works of Galen, furnish only imperfect information. Contentions, however, ran high between dogmatics and empirics, the former of whom were chiefly guided by reasonings, as the latter trusted solely to experience.

The Greeks, for many ages, were the only people we are acquainted with, who cultivated the sciences with industry. For near five hundred years they were almost exclusively the professors of the healing art. The Romans were cautious of admitting the refinements of learning into their state; and had

nearly attained the height of their glory, before they willingly received physicians into their city. Asclepiades the Bithynian, the contemporary and friend of Pompey and Cicero, appears to be the first, who practised medicine at Rome with any degree of reputation. This author left his native country, with the design of instructing the Roman youth in the arts of eloquence; an acquirement, which was held in high estimation among that warlike people; but not succeeding in this pursuit, according to his wishes, he soon discovered, that the profession of medicine offered a fair opening for the exercise of his talents. The state of the art, indeed, was then peculiarly propitious to his undertaking; the former fame of Hippocrates being divided between Erasistratus and Herophilus, and some authors of the empiric sect. Asclepiades was too discerning not to perceive, that new doctrines could not fail of drawing followers, and too enterprizing not to attempt to carry this purpose into execution. He probably, in the first place, read over all that had been written by former physicians; the most essential parts of which, he selected with a good deal of art, and so modified as to form a system, which appeared to be complete, and which exhibited upon the whole, considerable appearances of novelty. If we take the pains to trace his opinions to their sources, we shall find that Democrites or Epicurus furnished him with his philosophy, that Herodicus supplied him with the hints of bathing, friction, and gestation, that the plan of abstinence, or fasting for three days, was learnt from Erasistratus; and that Petro and Cleophantus instructed him in the practice of giving cold water, and of allowing greater indulgence in the use of wine.—But though the authors I have mentioned furnished Asclepiades with the hints of his doctrines and practice; yet we may observe, that he has always modelled these after his own fashion, and carried them

farther than had ever been done by their original inventors. It was remarked above, that the profession of medicine was divided into different sects before the time of Asclepiades ; yet, as far as we can judge from the imperfect information which has been transmitted to us, this author was the first, who deserves the name of an active physician. Previous to the time of this enterprizing Bythynian, medical men trusted the cure of fevers chiefly to the efforts of nature ; and were only solicitous about the mode of death. Asclepiades, assuming a bolder principle, endeavoured to cut short the fever in the midst of its course : and it is reasonable to suppose, that by stepping suddenly from the extremes of torture to the highest gratification of the appetites, such changes were effected in the state of the system, as probably sometimes accomplished the purpose. Such were the general views of this author. It may not be improper to add a few remarks on some of the remedies which he employed.

It is impossible to fix the date, when friction and warm bathing were first numbered among the assistances of the physicians. They appear, however, from the testimony of Celsus, to have been used with caution by the ancients. Asclepiades not only indulged in them with freedom, but so conducted the management of bathing, that it might be justly considered as a luxury of the highest elegance. Gestation, another of the gymnastic remedies employed by this author, even in an early period of ardent fever, is an experiment of a still bolder and more desperate kind ; and such as succeeding writers with one voice seem to have condemned :—perhaps without examination or fair trial. I am sensible that the opinion, which I am to offer on this subject, is not likely to meet with general approbation ; but the opportunity which I enjoyed, during the late war, of serving with a regiment, which was almost constantly in the

field, enables me to confirm the truth of it by ample experience. The good effects of gestation or travelling, even in awkward conveyances, were very obvious in almost every stage and situation of the ardent bilious fever; but I shall relate some instances of its success, which appear to preclude all possibility of doubt. At Ebenezer in Georgia, at a season, when the thermometer, in the coolest part of the house, often stood at ninety-six, and even sometimes rose above it, I was seized with the ardent bilious fever, which at that time made dreadful ravage among the troops. For six or seven days I did not once shut my eyes; my thirst was great, yet every sort of liquid, which I could procure, was nauseous; the distinction of paroxysm and remission was no longer perceivable; the pulse, at the wrist, was neither uncommonly frequent nor strong; but the pulsation of the descending aorta was so great, as to shake the whole frame; anxiety and restlessness were intolerable: in short, the torment was so excessive, that human nature could scarcely suffer more. The situation was precarious; and without much reflexion I indulged the desire of being carried to Savanna; though the distance was not less than twenty-five miles. An open carriage, the only conveyance which the country afforded, was provided for the purpose; and I was put into it, in a very feeble and distressed condition. Fortunately the day was cloudy, and cooler than ordinary. The roads were likewise soft and sandy. Though the carriage was very defective, the motion was no ways unpleasant; and I had not travelled two miles before I felt a wonderful increase of vigour. It rained heavily about half way, and before I reached Savanna, I was drenched to the skin. The effects which might have been expected, did not follow. Instead of being hurt, I was surprisingly benefited. I walked into the house with strength and firmness, eat something without dislike,

and slept sound the following night; in short, obtained a perfect remission of the fever. This is a strong instance of the good effects of travelling in fevers; yet it is only a solitary one. I shall add another, which places the fact on a still firmer basis. I mentioned in a former part of this treatise, that while the 71st regiment lay at the Cheraws, the endemic of the country prevailed among the men in a most unusual degree. The disease was often without distinction of paroxysm and remission; the anxiety and restlessness were intolerable, bilious vomitings and purgings were frequent, and excessive. While near two hundred men were in this situation, an order arrived for abandoning the post. It being impossible, as we were situated, to provide waggons to transport so great a number of sick, about forty of those who were least likely to be soon fit for service were sent down the river in boats. Of the particular fate of this party I cannot speak with certainty; but I have the satisfaction to add, that not a man died of those who retired to Camden by land; and that after the third day, scarcely a fever was left, which had not assumed a regular intermitting form. This appears at first sight almost an incontrovertible proof of the salutary effects of gestation; but I must not at the same time omit to mention, that benefit also probably resulted from a change, which accidentally happened in the state of the weather. The weather, (which, during the time we remained at the Cheraws was uncommonly hot,) became unexpectedly cool after the march was begun; together with rain, from which the sick men had nothing to shelter themselves. This instance of exposure to rain furnishes a proof of a fact of much importance. It is generally believed that getting wet with rain is hurtful to a person in health. It is no less commonly supposed to be certainly pernicious in sickness; but the contrary appears to be sometimes the case. I have seen the happiest effects from the application of cold, even

from getting accidentally wet with rain in many instances, besides the present. Increase of tone and vigour was generally the consequence; and life was evidently protracted, sometimes perhaps saved, by accidents, or modes of treatment, which, in the common opinion of mankind, would have been reckoned the causes of death.—But though I have mentioned the accidental good effects of gestation, and even of exposure to rain in different states of the ardent fever; I shall not be so paradoxical, as to recommend such experiments in common practice. I must however be allowed to observe, that we have little cause to be afraid of their pernicious effects. Motion and travelling, as far as my experience goes, were constantly hurtful in cases of local pain and inflammation; or in derangement of intellect; but, on the contrary, constantly serviceable in anxiety and restlessness, depending on the state of the stomach; as also in affections of the biliary system.

I mentioned before, that Asclepiades practised medicine at Rome with great reputation. He propagated his doctrines with a good deal of success; but such is, and ever has been the fate of our conjectural art, that no system has yet preserved its credit undiminished for any length of time. The views of physicians, with regard to diseases, had hitherto been various and complex; even fevers had been often considered, by the same person, as depending on different causes. Themison, a pupil of Asclepiades, attempted to remedy the perplexity which necessarily arises from this instability of arbitrary conjecture; and endeavoured to reduce all the disorders, to which the human body is liable, to two general classes;—viz. to those which arise from an increased degree of stricture, or its opposite affection, preternatural laxity; to which he afterwards subjoined some complaints, which appeared to partake of the nature of both.—The idea of simplifying diseases did not probably arise

in the mind of Themison, till the latter period of his life; on which account, perhaps, the doctrines were left in some measure unfinished; and it is to Theffalus, who lived in the time of Nero, that we are indebted for completing the methodic system, and for enlarging the bounds of its fame. Soranus likewise added to its credit. It is a misfortune, which we must regret, that except Coelius Aurelianus, there is not one of the many authors who were attached to the tenets of this sect, whose works have escaped the wreck of accident or time. This only remaining author appears to have copied, by his own confession, almost literally from Soranus.

It does not belong to this place to enter deeply into the tenets of the sect, with regard to diseases in general; but leads to views of some importance in the theory and treatment of many disorders. It proceeds on the supposition of circulation in all parts of the body; and with a little latitude may be supposed to comprehend the affections of the animated solid or contractile fibre. Fever is considered by the writers of this system, as a disease of the class of stricture; and if we had authority to add spasmodic, we might believe the methodics had discovered a very important phenomenon in the history of febrile diseases.—But this some may think is granting them too much. If we take the trouble to trace their doctrine, respecting the cause of fever, to its source, we shall find that the hints of it are furnished expressly by Asclepiades, who perhaps borrowed his ideas on the subject from Erasistratus, or even from Hippocrates.—But as stricture, assigned by the writers of the methodic sect as the cause of fever, is not perhaps radically different from the obstruction of preceding authors; so we do not find much material difference in their manner of conducting the cure. The followers of Hippocrates, Erasistratus and Themison proceeded equally on the idea of restoring permeability in the

minuter canals of the system; a purpose which they conceived would be best affected by certain processes of attenuation and relaxation. Thus Hippocrates diluted plentifully, and gave nourishment only sparingly, during the first days of a fever. Erasistratus enjoined a general abstinence, Asclepiades prescribed a term for the duration of the abstinence; while Themison limited it so rigidly to the space of three days, that the practice was distinguished by the name of diatriton, as its followers were known by that of diatritarii. This idea of diatriton constituted an object of much importance in the system of the methodic physicians. It directed all their movements, and is the only view, which can properly be called their own. The mode of application of the remedies of preceding authors was occasionally modified by this sect; but except that which I have just mentioned, we do not discover much that is fundamentally new.

It may not be improper in this place to take notice of the practice of cold bathing in fevers, which was introduced at Rome in the infancy of the methodic sect; and which afterwards acquired great celebrity in different parts of the world. The Emperor Augustus, who for the greatest part of his life was afflicted with ill health, was at last attacked with a complaint of so obstinate a kind, that the usual applications did not afford him any relief. Warm bathing and all that train of remedies had been tried in vain. The Emperor was sensible of his desperate situation; and his physician Antonius Musa, baffled in all his attempts, ventured, though apparently at great hazard, to prescribe the cold bath. The health of Augustus was unexpectedly restored by it; and the physician was highly honoured, and amply rewarded. It is a misfortune that we do not know the specific nature of the disease, under which the Emperor laboured; but we have reason to conclude, from the circumstances which attended the cure, that it

was a fever of a bilious remitting kind; perhaps complicated with catarrhal affection, and wasting of the body. But though the success of cold bathing, in the instance I have mentioned, was much greater than expectation; yet the remedy did not long retain its credit. It was soon afterwards employed in the case of Marcellus, a youth of great hopes, and presumptive heir to the empire; but the event proving unfortunate, it fell suddenly into disrepute,—probably without sufficient cause. We find however, that Celsus, in less than half a century, afterwards ventured to recommend it in a certain species of the slow or hectic fever; though Charmes, a physician of Marseilles, appears actually to be the first who rendered the use of the remedy general. Galen employed it frequently with great freedom and boldness. The Arabians, particularly in pestilential diseases, went still farther than the Greeks or Romans; and we presume, from the fact recorded by Busbequius, that it was sometimes prescribed at Constantinople, even so late as the sixteenth century. I do not know that it has been often tried in Europe, since the revival of the medical sciences in the West. I mentioned in a former part of this treatise, that I had employed it frequently in the fevers of Jamaica; I now add, that I have ventured upon it in the fevers of this country with so great success, that I should expect the most beneficial effects might result from a proper management of it. (2)

We are indebted to Celsus, who lived in the time of Tiberius, for preserving many of the opinions and practices of preceding physicians, which otherwise would probably have been lost. This author, not less remarkable for candour, than for the elegance and perspicuity of his manner of writing, does not seem to have been blindly devoted to the tenets of any particular sect. He has favoured us with valuable extracts from the works of the most celebrated dog-

matics; he has likewise taken notice of the most remarkable opinions and practices of the Empirics, without omitting to mention the innovations of Asclepiades and Themison. With regard to his particular merit as a physician, we may observe that he every where discovers an excellent judgment, and that his practice is generally decided without being rash.

So great have been the ravages of time or accident among the writings of the early physicians, that it is scarcely possible altogether to avoid error in attempting to trace the various revolutions in medical practice, prior to the time of Galen. From that downwards we are enabled to give a more certain and better connected view of the subject; there being few of the principal writers of this latter period, who have not been preserved entire. When Galen came first to Rome, which was in the time of the Emperor Antoninus, the practice of medicine was chiefly in the hands of the followers of the methodic system. The practice of diatriton, or abstinence for three days was then in high fashion, not only with the professed pupils of Thessalus, but even with the few remaining adherents of Erasistratus. Galen every where declares himself its inveterate enemy; and often transgresses the bounds of liberality and decency, in his attempts to turn it into ridicule. His own endeavours are expressly exerted to revive and establish the principles of Hippocrates; and to complete those parts, which the want of time or the want of experience of his master had left imperfect. He possessed uncommon fertility of genius, a great flow of language, and a judgment by no means deficient; yet, from a sophistical spirit of philosophizing, he frequently so entangled his opinions with theoretical distinctions, that his views are often uncertain, and sometimes embarrassing. The principle with which he sets out is directly to oppose the actual existence

of fever; he next recommends to remove, at least to avoid an increase of those causes which give rise to the disease. These ideas are drawn from the writings of Hippocrates, and are such as no person will dispute: but, as the causes of fever are supposed, both by Hippocrates and Galen, to be many and various, so the indications of cure often require to be executed in different, and sometimes in directly opposite manners. This necessarily gives rise to confusion; and entangles the practitioner in the mazes of doubt and conjecture; to obviate which, as much as possible, the learned commentator of Hippocrates has thought fit to divide fevers into three general kinds, viz. ephemeral, continued, and hectic or habitual; the causes of which he supposes to be so little analogous to one another, as to demand particular management in the method of cure.

We look in vain for new views, or material improvements in the management of fevers, in the writings of those Greek physicians who followed Galen. Oribasius professedly is no more than a collector of the opinions and practices of other men; and Aetius, on the present subject, does not aspire to much higher fame. There are, indeed, few of his observations, which may not be found in the volumes of Galen, or some preceding writer; yet he seems generally to have comprehended what he wrote. He digested the knowledge which he found in books with care and judgment; and gives an arrangement so clear and perspicuous, that the person may derive information from Aetius, who would be overwhelmed and lost in the prolixity of Galen's discussions.

From those writers, however, who trod implicitly in the footsteps of Galen, we must be allowed to separate Alexander of Tralle, a physician who lived in the sixth century. This author wrote his book on fevers at a very advanced age; and though the treatise perhaps does not contain many ideas, which may

not, in some shape or other, be found in the writings of his predecessors, yet the observations have the appearance every where of having originally arisen from actual experience. The language, which is concise, clear and perspicuous, is wholly his own. The ambiguous circumstances of diseases are more accurately discriminated than in any preceding work which has descended to the present times; and though the manner of accounting for things may be sometimes erroneous, yet it has had little influence on the practical indications, which are almost unexceptionably judicious. As Alexander of Tralle wrote at a time of life when fame must have been indifferent to him, and to a friend, whom he was more solicitous to instruct than to amuse with the splendour and variety of his learning, we have an additional cause to give our confidence to his observations. His manner is candid and ingenious; and the treatise before us may be considered by the practical physician, as the most valuable of the remains of the ancients. Judicious cautions are every where interspersed, and considerable changes in the management of remedies are sometimes attempted; but the practice of giving opiates in a certain state of fever is the only practice of this author, which has any title to be called innovation.

Paulus is the next physician of note, who lived after the days of Galen. He was born in the island of Aegina, and travelled over many countries. It is probable that he was sufficiently acquainted with every discovery, which had been made by his predecessors; yet Galen, on the subject of fever, is the author whose works he has principally followed. His book on fevers, indeed, contains all the material doctrines and observations of that voluminous writer; and those who dread the labour of encountering the prolix and sophistical disquisitions of the commentator of Hippocrates, may find a very distinct analysis

of his opinions and practices in the treatise of Paulus Aegineta.

Having endeavoured in the preceding pages to give a short view of the methods which were usually pursued by the most eminent of the Greek physicians, in the cure of fevers, it will be necessary in the next place to take some notice of the improvements of their immediate successors, the Arabians. This task will be soon performed; the Arabians have not in reality opened any views in the curative indications of febrile diseases, which were unknown to their predecessors; or which require that we should spend long time in endeavouring to explain them. The medical science evidently drew its origin from the East; yet it was also soon reconveyed to the countries from whence it sprung, with improvements and additions from the genius of the Greeks. We learn from Herodotus, that Democedes, a native of Crotona, who had studied medicine in the island of Aegina, far excelled all the physicians of the Persian court, even so early as the time of the first Darius; though the court of this Prince probably could boast of all the skill, both of Assyria and of Egypt. Ctesias sometime after was held in great estimation by Artaxerxes; and the invitation, which was held out to Hippocrates by the Persian monarch, indicates very clearly, that the Greeks, even then, were more famed for medical skill than the inhabitants of the Eastern countries. The islands and shores of the Mediterranean seem through the whole history of medicine, to have produced the greatest number of physicians. Crotona and Cyrene were famous for several ages: and Alexandria, at a later period, rose into great celebrity. Students flocked to it from every part of the world; it was even necessary that every one, who aspired to wealth or reputation in physic, should spend some time in this celebrated seminary. It was owing perhaps, in some degree, to the vicinity of

this illustrious school, that the province of Syria enjoyed at one time, a considerable share of learning and learned men. The works of the most eminent of the Greek physicians were translated into the dialect of the Syrian country, in the seventh and eighth centuries; by which means they were probably, in some measure, propagated in the East: though we also are informed by Abulpharage, an Arabic writer, who had preserved many curious anecdotes of private history, that the doctrines of Hippocrates were planted in the Chorasan, at a still earlier age, by the physicians, who followed in the train of Aurelian's daughter, who was married to Saporess king of Persia: nor is it improbable, that these doctrines were still more generally diffused through the Persian dominions, by the alliances of friendship, as well as by the long wars, which were afterwards carried on between the Greek empire and the celebrated Khorrou Pawiz. But though the inhabitants of Syria and Irak were an enlightened nation, at an early period; their neighbours, the Arabians, who afterwards attained so great a name in science no less than in war, remained long in a state of illiterate ignorance. Before the establishment of islamism, there scarcely was a native Arab, who could either write or read. The little genius they possessed was chiefly exerted in composing verses, or in colouring a rhetorical harangue. They appear, indeed, to have acquired some practical knowledge of the motions of the heavenly bodies; and it is likewise reasonable to suppose, that they had the same skill in medicine, as is common to savage nations; but there is no reason to believe, that they, as yet, had made progress in the medical art, considered in a scientific view. Hareth, a native of Tayef, who lived in the time of the prophet, and who seems to have been in habits of intimacy with that singular man, is the first of the Arabs, whose name is recorded among the

physicians of the East. This person, who acquired some knowledge of medicine at Nisabour, and other places in the Chorasan, returned home after some time, with great wealth, and no small share of fame. He practised among his countrymen with much reputation; but how far he spread the light of science among them is uncertain. The Saracens advanced rapidly in conquests and the establishment of their faith; but we do not hear any thing of their progress in the healing art, till the ninth century. Syrians and Persians, generally of the Jewish or Christian religion, laboured sometimes for the warlike Arabs in the servile occupation of curing diseases, at least we do not know that any of the Saracens attained much eminence in medical science, till the translations of Honain and his pupils laid open to them the treasures of the Greeks. We are ill qualified at this period to judge of the merit of these translations. But if we may be allowed to form conclusions, from the use which has been made of them, we shall not, perhaps, be disposed to entertain a very high opinion of their accuracy. In many instances, the later Arabian physicians have expressed the ideas of Hippocrates and Galen only very loosely; and in some few cases, perhaps, have not very clearly comprehended their meaning. But, as the later Saracens were seldom skilled in any language except their own; the original translators are probably alone blameable for the whole of these mistakes.

The medical authors, who wrote in the Arabic language between the ninth and fifteenth centuries, and, who still lie concealed in the less accessible dress of their native country, are almost innumerable: neither are those, who have been introduced into the common acquaintance of Europeans few in number, or small in volume. If I possessed a complete series, even of those who are commonly known in Europe, the examination I have entered upon might be drawn

out to a considerable length; but as I have no hopes of obtaining that soon, I shall content myself with giving some idea of the Arabian system of practice in fevers, from the works of Avicenna, the most eminent and best known of the Oriental physicians. An examination, indeed, of one of the writers of this nation may, in a great measure, render an inquiry into the others unnecessary. Those, at least whom I have seen, do not differ materially from one another; or perhaps essentially from the Greeks who went before them. The canon medicine, the principal work of Avicenna, exhibits a systematic view of the whole art of medicine, theoretical as well as practical. I have read over with care all that relates to fevers; and though there is little, perhaps which may not ultimately be traced to Galen or Hippocrates; yet the author has not copied servilely from either of them. He is more full and particular than the one; less prolix and tedious than the other. I must however remark, that the distinctions and divisions, which he has attempted to introduce into the history of fevers, are not only unnecessary, but actually serve to embarrass the indications of cure. His general theories are those of Galen. In the general conduct of the cure, he treads in the footsteps of the same master. He appears, indeed, to be more fearful of the lancet; while he is not perhaps always judicious, or consistent with himself, in the manner of employing it. On the contrary he has admitted cool air rather more freely, and has perhaps carried cold drink even to a bolder length, than had been done by the Greeks. Cool air, cold drink, and even the external application of cold, may be reckoned among the most effectual remedies in the fevers of hot climates; and this author has conducted the management of them, in a luxurious, elegant and efficacious manner. But though the works of Avicenna furnish a general view of the practice of the Arabian school of physic, it is still in some degree a defective one. As he has not

furnished us with a detail of the case of an individual, we are not able to judge precisely of his powers of discerning the disease, or of his decision in the manner of treating it.

The medical science, which after the taking of Alexandria was little cultivated by the slothful Greeks, or barbarous nations of the West, sprung up with new vigour in the province of Syria, in Irak and Arabia; and followed every where in the train of the Saracen conquerors. Extending with their arms over the northern coasts of Africa, it soon found its way into Spain; and, even so early as the eleventh century, was conveyed to Salernum in Italy, by Constantinus Africanus, a native of Carthage, who had lived long in Asia, and who was well acquainted with the language and medical knowledge of the Orientals. The Arabians were the first who opened the sources of chemistry; they also made great improvements in the art of surgery, and even described some complaints which in earlier ages were not taken notice of but they departed but little from the system of the Greeks in the management of febrile diseases. After the fall of the Roman empire the genius of learning made no exertion in Europe for a very long period of time. The native European slothfully acquiesced in the imperfect knowledge of Arabian writers, which was obtained from the inelegant, and perhaps often unfaithful translations of the Jews, who, for a considerable time, were no contemptible professors of the medical art. But, though some part of the knowledge of the Arabian physicians was communicated, in this manner to the nations of the West, in the eleventh and twelfth centuries; yet a part of the sixteenth passed over, before it was possible to trace any marks of improvement. Commentaries were written without number; but, for many years, there scarcely was an individual in all the seminaries of Europe who dared to think for himself. It has been customary to date

the revival of sciences in the West from the taking of Constantinople, by which the stores of Greek literature were in some degree opened to the world. The language of Galen began then to be more generally understood, and the writings of Avicenna fell rapidly into neglect; yet the advantage which accrued to medicine from the change, does not appear to have been great. The mind was exercised in a wider field of learning; but it was still in chains to the authority of the ancients. The opinions of Galen and Hippocrates were copied, recopied and commented upon by hundreds; but there were very few who ventured to use any judgment of their own. Among the most celebrated of the followers of Galen we may reckon Fernelius, Forestus, Lommius and Sennertus, men of considerable talents, but who were too scrupulously devoted to the principles of their master, to open a new road in the practice of the art. This was reserved for Paracelsus, who early in the sixteenth century ventured to attack the opinions of his predecessors, and the authority of Galen. Paracelsus possessed a consummate share of assurance, together with knowledge of remedies which were not generally known at that time. He acquired some acquaintance with the chemical discoveries of the Arabians, in the course of his various travels, and applied in practice what he had learned, on his return to his native country. He despised the authority of the regular physicians, employed remedies with great boldness, and often with singular success. This success was even very probably exaggerated by report; and there appear to have been many, who followed him implicitly; while others exerted themselves in modifying and improving his ideas. Under this last view we may rank Van Helmont, a person, who effected a very material innovation in the manner of curing febrile diseases. Van Helmont possessed considerable learning; but discovered, at the same time such marks of warmth and enthusiasm of

genius, as diminished his credit with contemporary and succeeding practitioners. The terms which he employs, are sometimes ridiculous; and his reasonings are frequently disfigured with fancy and whim; yet his ideas are generally important, and often well founded. The archæus of this author does not differ materially from the sentient principle (*τα εννοηματα*) of Hippocrates; and perhaps comprehends the whole idea of the *vis medicatrix naturæ* of the moderns. Van Helmont proceeds to the cure of fever on the important principle of exciting, or calling forth the powers of life, to exterminate an offending cause; so that we may actually consider him as the first, after Asclepiades, who attempted to take the business wholly out of the hands of nature. He disregards the processes of coction and crisis; and makes a decided effort to cut the disease short at an early period. He is likewise an enemy to bleeding, purging, vomiting, and the various evacuations which had been employed by his predecessors, attempting to accomplish his purpose solely by the means of sweat, and insensible perspiration. The success of his practice was so great, that he deems the man unworthy the name of physician who suffers a fever to exceed the fourth day; a degree of success, which all the powers of antimony have not yet enabled us to boast of.

The circulation of the blood having been proved incontestably about the middle of the last century, hopes were reasonably entertained, that the healing art would be benefited by the discovery. It does not however appear that medical men, for some time at least, either argued more clearly, or practised more successfully. The advocates of the galenical and chemical schools had gradually approached to each other; so that the doctrines and practices of those contending parties were now insensibly blended together. Sometimes the one mode of thinking predominated, sometimes the other; but chemical principles every

where gave scope to the imagination, which often indulged in the wildest extravagance of conjecture. Among the number of those conjecturers, who arrived at much eminence and fame, we may reckon Sylvius de le Bae, who lived in the end of the last century, and introduced a considerable innovation in the manner of treating fevers. His theories are generally known. They appear to be totally destitute of foundation; yet unfortunately are the groundwork of all his practical indications. His principal view consists, in regulating the mixtures of bile and pancreatic juice. He likewise lays so great a stress upon the nature of the occasional cause, as gives rise to doubt and ambiguity. Thus he sometimes prescribes acids, though oftener aromatics, volatiles, and opiates. But as we possess some cases, which he appears to have healed, in the Leyden hospital, with all his skill and attention, we are enabled with more certainty to form a judgment of the particular merits of his practice. It has not any claim to extraordinary success; yet it is evidently innocent of the great harm which some later authors have imputed to it. In short, if we except opiates, we may consider the rest of his remedies as very feeble and ineffectual.

During the time that Sylvius flourished in Holland, a new theory of fevers was offered to the public in England by Dr. Willis, the celebrated author, to whom we are so much indebted for bringing into view the importance of the nervous system, in the economy of the human frame. It does not however appear, that this writer's theory ever extended far, or that it was the cause of much innovation in practice.

The method of treatment, which was generally adopted in the fevers of England, at the time when Sydenham began to study medicine, consisted principally in bleeding, in vomiting with antimonials, in evacuating the intestinal canal by means of glysters or gentle laxatives; and, in the latter periods of the

disease, in attempting to raise sweat by hotter alexipharmics. In the first constitution of seasons described by this author, viz. the years 1661, -62, -63, -and 64, we do not find any material deviation from this general plan of cure; which was the plan followed by Willis, and other contemporary physicians. In the next constitution, viz. the years 1667, 68 and part of 69, Sydenham forces himself on our observation by an attempt to effect a very important innovation. The fever which prevailed during the last mentioned years was generally of long duration. It was usually accompanied with profuse sweatings, and often distinguished by petechial eruptions. Cordials, and hot regimen were sometimes observed to cut short its course abruptly; yet dangerous symptoms were still more frequently the consequence of this stimulating mode of treatment, than a favourable termination. The sagacious Sydenham, instructed by repeated experience of the bad effects of this common method of cure, adopted a contrary one; which he pursued with boldness, and apparently with great success. It may not be improper to observe in this place, that our author is not to be considered as the inventor of the antiphlogistic method of treating fevers. The ancients, particularly the Arabians, carried the cooling system still farther than the moderns. About this time however it had fallen into general neglect; and Sydenham undoubtedly possesses the merit of restoring it; more perhaps from his own observation, than from a knowledge of what had been done by his predecessors. Part of the year 1669, the years 1670, -1671 and 1672, form another constitution of seasons, according to this author's arrangement of diseases. The epidemic assumed a different appearance from the former. It was chiefly distinguished by symptoms of dysenteric affection. Our author, however, still adhered to the outlines of the antiphlogistic plan; and treated the disease successfully with

bleeding, and the repeated use of laxatives. The method of treatment, which he adopted, admits of a remark. In the former epidemic, the profuse sweatings were checked; in the present, the intestinal evacuations were encouraged; in one case he appeared to promote, in the other to thwart the intentions of nature; practices so opposite that we cannot easily reconcile them. The next constitution, viz. the years 1673, -74 and 75 discovered a fever with a new train of symptoms, and in Sydenham's opinion of a very different race. It was principally distinguished by pleuretic and rheumatic affections, by coma and stupor. The general antiphlogistic practice was still persisted in; and the whole of the cure was trusted to discretionary bleeding, blistering the back part of the head and neck, with the repeated employment of glysters. The hotter diaphoretics were cautiously avoided. In the year 1684, this diligent observer imagined he discovered the appearance of a fever of a perfectly new and unknown kind; a fever accompanied with more or less derangement of intellect, and many other symptoms of nervous affection; the species of disease, perhaps, which nosologists have distinguished by the name of Typhus. But though this species of fever was supposed by our author, to be extremely different in its nature from any that he had yet seen, we do not however perceive, that this idea suggested to him any material difference in the mode of treatment.

From the short view which has been given of Sydenham's practice in fevers, it is easy to perceive the rise and progress of the method of cure which he adopted. Antiphlogistic processes were carried to a greater length by the ancients, than the moderns have yet dared to risk. But there is little reason to suppose, that Sydenham owed the ideas of the alterations which he introduced to information from preceding writers. His practice bears every where authentic

marks of having arisen from his own observation.--- The most common termination of fevers, is by sweating or increased perspiration; a fault observed by Van Helmont, and which furnished that author with the idea of prosecuting the cure of the disease wholly on this plan. The practice seems to have been early adopted in many parts of Europe; and it even continued in general reputation in England, at the time that Sydenham began his medical studies. Sweating undoubtedly is often beneficial, and may be considered, upon the whole, as the most certain means of exterminating the cause of fevers; yet bad effects often resulted from it---then probably more owing to the manner in which it was conducted, than to the real hurtfulness of the thing itself, viewed in the light of a general remedy. Sydenham, who does not appear to have discriminated between the actual effects of sweating and the effects of the manner of exciting it, condemns the practice in general terms, and passes to an opposite method of treatment with a good deal of boldness. It has ever unfortunately been the fate of physic, like every other conjectural art, to pass from one extreme to its opposite by large strides; and thus, even the sagacious Sydenham, who had seen the bad effects of treating remedies in fevers with much of the inflammatory diathesis, was induced to employ antiphlogistic processes in those species of disease, which we should be disposed to believe do not well admit of them. The new, or nervous fever, in the opinion of the practitioners of the present age, could not well bear the plentiful evacuations prescribed by this author; at least, we may safely affirm, that such evacuations are not by any means necessary.--- But I shall dismiss this subject with observing, that the practice of Sydenham, if we except the article of bleeding, can only be considered as feeble, and as often insignificant. His remedies sometimes, perhaps, obviate the fatal tendency of symptoms; but are not

capable of having any decided effects on the natural course of the disease. I may likewise add, that his practice is directly at war with the principle of his theory. If fever is considered an effort of nature to exterminate something hurtful from the constitution, bleeding and those evacuations, which diminish the powers of life, are not the proper means of effecting this purpose. But the truth is, the practice of Sydenham was his own; his theory was that of the times in which he lived, formed from a mixture of the doctrines of Van Helmont, Campanella and Dr. Willis.

It may not be suspected, perhaps, from the remarks which I have made on the practice of Sydenham in fevers, that I do not consider him as the author of so much essential improvement, as has been generally imagined. I must however acknowledge, that he deserves the highest praise for the accurate and well discriminated history of acute diseases, which he has left us. The descriptions are complete, and the circumstances so peculiarly chosen, that the disease itself is actually before the eyes of the reader. These are the great, and as yet the unrivalled excellencies of Sydenham; but in admitting such essential differences in the cause of epidemics as he has done, he necessarily leads us to embarrassment, and often leaves the practitioner in a state of uncertainty. The disease described by Sydenham, in the various constitutions of seasons between the years 1661 and 1685, shews external marks of considerable diversity; yet I must confess, that I see but little reason for supposing, that these appearances arise from causes which are totally and fundamentally distinct. The fever of Sydenham, in all its forms, is in fact the common endemic of England. Circumstances however often arose then, and still arise, which modify the general cause in such a manner, that the disease appears at one time with symptoms of inflammatory diathesis, at another with

symptoms of nervous affection, and at another, with a general disposition to affections of particular organs. These modifying causes, which are more general or particular, more obvious or obscure, often continue for a certain train of seasons, and influence very materially the character of the reigning epidemic. The general cause of the fever is in reality one and the same, yet I must also acknowledge, that the modifications are evidently many and various, and often very remotely different from each other.

Chemical principles for some time past, had the principal share in enabling medical writers to account for the phenomena in fevers; but about the end of last century, the mechanical philosophy was again revived, and being incorporated with the doctrines of the chemists, the laws, and various derangements of the human frame, were then explained on the principles of hydraulics, or chemical mixture. The authors who adopted this mode of reasoning were numerous, and some of them were of great eminence; but at present I shall only take notice of one of the greatest of them, the celebrated Boerhaave, who formed a system, which was considered as the most perfect that had hitherto been offered to the public. The doctrines of this author acquired uncommon fame. They soon extended over all Europe, and, indeed, still prevail in the greatest part of it. But though Boerhaave has presented us with a methodical explanation of the phenomena in fevers; and has detailed the method of cure with clearness and precision; yet we do not find, that he has furnished much that is new and original in practice. He is every where cautious, and in most instances judicious; though he has committed a principal error in forming indications of cure, from a supposition of lentor and visciditv; a cause the very existence of which we have every reason to doubt.

During the time that Boerhaave flourished in Hol-

land, indeed before this author had arrived at much reputation, Professor Stahl, at Halle in Saxony, proposed some new opinions, which acquired considerable fame, and which have been considered, in some manner, as forming a peculiar system. The leading principle of this author, as is confessed by all, admits only of a feeble and inactive practice. I might even add, that it frequently leads to a pernicious one. Those tumults, or sufferings, which pass by the name of the efforts of nature, are extremely deceitful; and have obviously, in many instances, a destructive tendency. I mentioned before that they are trusted with danger; yet Stahl, proceeding on this principle, boasts extraordinary success in the cure of the petechial fever, which prevailed in most parts of Saxony towards the end of last century.

In a review of those authors, who have written on febrile diseases, it would be unjust to omit mentioning Hoffman, contemporary with Stahl, and professor in the same university. The actual alterations which this author has introduced into the cure of fevers, are not perhaps very great in themselves; yet his important discoveries, in regard to its theory, entitle him to great consideration. The most of the remedies, which he employed, are found in the writings of his predecessors, or contemporaries; yet they were not, perhaps, always prescribed by them with the same intentions. The theory of Hoffman opens a road for the trial of antispasmodics, merely on the footing of antispasmodics; a class of remedies of much importance in the cure of febrile disorders. In practice, Hoffman is more decided than Stahl; and his views, perhaps are more extensive than those of Boerhaave. He is likewise uncommonly candid; and has furnished us with a great variety of histories, which serve in many cases to illustrate the nature of the disease.

The antiphlogistic method of treating fevers, the

ground-work of which was laid by Sydenham, and improved by Boerhaave, prevailed in most parts of Europe, without material alteration, till near the present times. Blistering with cantharides, which had been employed with caution, and which was even suspected of deleterious effects by many, was introduced into practice in the end of the sixteenth century, and about the beginning of the present began to be, as employed, a common remedy in many species of fever: its good effects were often obvious, and, according to the prevailing mode of reasoning, were supposed to arise from a quality which cantharides were believed to possess, *of attenuating* the blood. This mode of operation is no longer admitted; but the remedy still retains its credit. Few people pretend that blisters are possessed of specific powers in shortening the course of fevers; yet every one allows, that they obviate many symptoms of dangerous tendency, and that they often dispose the disease to assume its proper form. In fevers, accompanied with local affection, their beneficial effects are universally acknowledged; and, even in many cases of general irritability, they often produce very fortunate changes. But I must observe, with regard to this, that much depends on management, and the mode of application. In local affections the local application is most effectual; in cases accompanied with much general irritability, the back part of the head and neck ought, perhaps, to be preferred to others. I have thus frequently seen in fevers, where there was much general irritability, that blisters applied to the extremities evidently aggravated the disease; while I have also observed, that they as certainly diminished the hardness and frequency of the pulse, and disposed the patient to rest, where they were applied to the back part of the head and neck. There is another remedy that I shall take notice of before leaving this subject, which possesses still higher reputation than blisters.

Antimonial preparations have been employed occasionally in fevers for many years past; but they did not gain established credit in this country, till within these thirty years. The discovery of the famous powder of Dr. James appears to have been the cause of a considerable innovation, in the manner of treating febrile diseases. The practice of Boerhaave did not go farther than to obviate symptoms of fatal tendency; it left the disease to pursue its own course. Dr. James assumed a bolder ground, and promised to cut short the fever abruptly by means of his powder. There are many who still tread in his footsteps; I acknowledge, as I have hinted before, that their attempts may be often successful in the early stages of the illness, or often useful towards a critical period. I cannot however believe, that this powder, or any preparation of antimony with which we are yet acquainted, possesses the power of abruptly terminating a fever wherever it is employed; at least, to effect this requires a management of which I confess myself ignorant. The effects which Dr. James promised from his powder, others have attempted to obtain from emetic tartar; but I have reason to think with inferior success.

The wonderful power, which the Peruvian bark is observed to possess, in suspending the course of intermittents, has led the practitioners of the present times to employ it, with the same views, in fevers of various denominations. But after what I have said of the uncertainty of its effects in checking the course of the remitting fever of Jamaica, it will be needless to repeat here, that I do not expect to find it of much efficacy, in shortening fevers of a more continued kind. I must, however confess, that, even in many of these, it is a remedy of great value. It supports, in a very eminent degree, the tone and vigour of the powers of life.

Opium has been prescribed occasionally in fevers

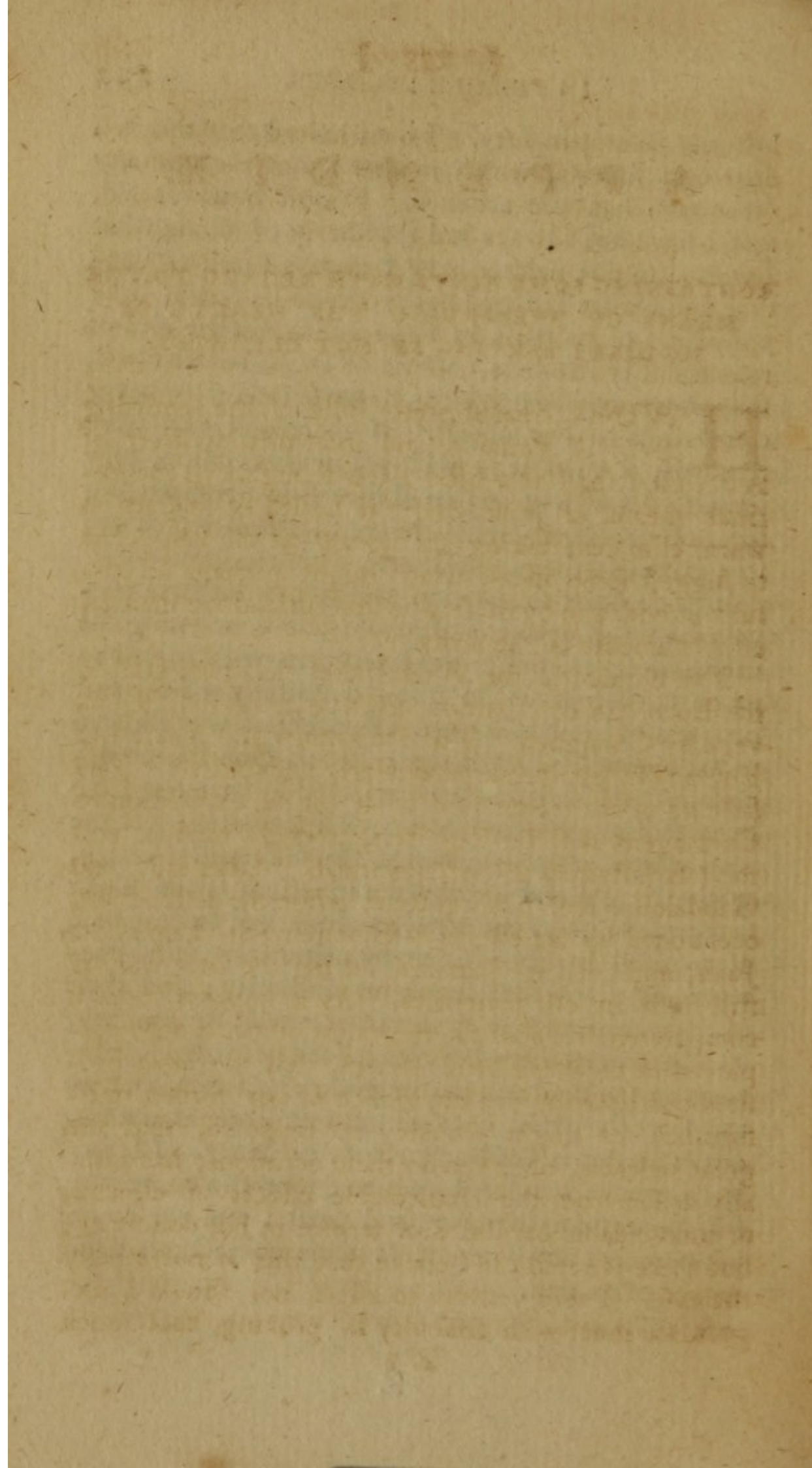
for a long time past; but it is only of late years, that it has been recommended, as a general remedy in some particular species of this disease. The practitioners of the West Indies, prescribe opium with more freedom, than is generally done in England. It is frequently employed to mitigate symptoms; and in some situations which were very alarming, I have given it in very large quantity with unexpected good effects. In the slow fevers of this country I have frequently had recourse to it; and, combined with antimonials and camphire, have found it to be a remedy, of great value. Opium in general was more cordial than wine. In cases of despondence and distress it gave a confidence to the mind, and imparted a pleasureableness to the sensations above all other remedies. In short, it appeared often, not only to be instrumental in conducting the disease to a favourable termination, but it enabled the patient to pass through it with comfort to himself.

I have mentioned in the preceding pages, the most eminent of those authors, who have written on the cure of fevers; giving at the same time such extracts from their works, that the reader, who has not the opportunity of consulting the originals, may be enabled to form some idea of the successive changes, the improvements, and oftener perhaps the corruptions, which have arisen in the method of treating febrile diseases, from the earliest records of the art to the present times. The apparent changes are more numerous than the real ones; while the most opposite modes of treatment do not often appear to have much perceptible effect on the event. The cure of fever has been hitherto pursued on two general and opposite views, viz. on the idea of exciting the powers of life, by means of heating and stimulating remedies; or of diminishing the reaction of the system by evacuations and other antiphlogistic processes. The above extremes of those directly opposite modes

of treatment have approached gradually to each other, or been variously combined by different practitioners. It cannot however fail of appearing strange to a person, who views the science of medicine in a philosophical light, to hear one set of men asserting that the proper cure of fever consists in exciting the powers of life, or in enabling nature to expel the disease by force; while another, with no less confidence, maintains that the plan of moderating or diminishing increased action is that which ought alone to be pursued. From such contradictory assertions we cannot easily avoid concluding, either that the most opposite means produce the same effect, or that nature has a prescribed mode of proceeding in fevers, which ordinary medical assistance is not powerful enough to controul. There are many eminent practitioners, who have been conscious of this truth. The candid Sydenham himself acknowledges, that those, whom he treated with all his skill and attention, and who possessed all the comforts that affluence could afford, did not often fare better than the poor, who were only sparingly furnished with necessaries, and who met with little assistance from medicine. I have myself seen many examples of the same kind. Sometimes I pursued the usual methods of cure with care and perseverance; sometimes I left the business almost entirely to nature, and I cannot say, that the difference of the event gives me much cause to be vain. But though I may appear to be sceptical with regard to the effects of common practice, I still cannot help being of opinion, that we may arrive at a high degree of perfection in the management of febrile diseases. So sanguine, indeed, are my expectations, that I cannot easily forgive myself, when the event of this disease happens to be unfortunate. The remitting fever of Jamaica is not a disease by any means devoid of danger; yet I should not be satisfied with myself, from the view which I now have of the subject, if I

lost one patient in fifty. I own indeed that this is a degree of success, which neither I, nor perhaps any other man has yet attained. I must however add, that I have not always had the liberty of doing what I wished to do; neither have I always dared to venture upon what I judged not proper to be done. The prejudices of patients in some cases, and the idea of responsibility in others, confine us to the beaten track, though we may be conscious in ourselves that it never can lead us to our object. If these obstacles were removed, a man who will act with decision, may promise almost any degree of success in the remitting fever of the West-Indies, in constitutions which are free from habitual complaints.

The constant fluctuation which has hitherto prevailed in the opinions of physicians concerning the causes of fever, and in their practices with regard to its cure, oblige us to think doubtfully of the real progress of the healing art. Hippocrates was allowed to have practised with more success than his predecessors. Aesclepiades was believed by many to have been still more fortunate than Hippocrates; yet the road which he pursued was totally different. Galen, who reviewed and improved the system of the Coan sage, rose to great eminence, and marked out the path of medical practice for many centuries. The doctrines of Paracelsus shook his authority; and these in their turn gave way to newer modes of thinking. In this manner there have been such perpetual revolutions in the modes of treating febrile diseases, that we can scarcely avoid concluding, that little or nothing of the matter is yet known with certainty. Medical writers have wandered from conjecture to conjecture, for more than two thousand years; and we do not yet perceive any prospect of these conjectures being nearer to an end.



A P P E N D I X.

CONTAINING SOME HINTS WITH REGARD TO THE
MEANS OF PRESERVING THE HEALTH OF
SOLDIERS SERVING IN HOT CLIMATES.

HAVING treated pretty fully of the remitting fever of Jamaica, and intermitting fever of America, I shall now offer a few thoughts on the various means of preserving the health of soldiers in warm climates; taking the liberty at the same time to suggest some ideas, which might perhaps be usefully attended to by those who superintend the medical establishments of the army.

The climate of the West-Indies has been fatal to the European constitution, even since its first discovery by Columbus. To the armies and navies of England it has been particularly destructive. The sad fate of the troops who went on the expedition to Carthagena will be long remembered; neither will the loss sustained at the Havannah, Martinique and Gaudaloupe soon be forgotten; while the destruction, occasioned by the effects of climate at St. Lucia, St. Juan, and even in Jamaica, during the late war, is still fresh in our memories. As it appears from a comparative view of the mortality of the troops employed in these different services, that we have profited but little by the experience of our former misfortunes, it might probably be supposed, that the great sickness, observed on these occasions, has actually arisen from the irremediable effects of climate, or unavoidable hardships of service in hot countries; but there is reason to believe that this is not wholly the case. I will venture to assert, nor should I expect to meet with difficulty in proving, that much

of it has proceeded from the inexperience or inattention of those who conducted the expeditions, or from such errors in the medical departments as might have been easily obviated. It is superfluous to observe, that the health of the soldier is an object of principal importance in ensuring the successes of war. We have many instances of expeditions apparently well concerted, which have failed from the excessive sickness of the troops: and too many proofs of this sickness proceeding from a neglect of such precautions, as might have contributed to the preservation of health. I have accustomed myself to look at this subject for more than fifteen years. I have turned it often in my mind, and cannot discover that much judicious attention has yet been paid to it. We cannot often perceive that health has been an object of consideration, in fixing the permanent stations of troops; or that it has been much regarded in choosing encampments in the field. Exercises, which might inure the body to hardships, have not been sufficiently enforced; and such sorts of diet, and such modes of life, as might obviate the danger of diseases, have been little attended to; while the best regulations for a speedy and decisive plan of cure do not appear to have been adopted. I shall be obliged, in tracing this subject, to advance some ideas which are contrary to the opinions of some celebrated authors, which combat popular prejudices, or which interfere with the views of interested men. I may be reckoned presumptuous perhaps in censuring freely; but I am conscious that I do not advance any thing which has not truth for its foundation.

It has frequently been the practice, in times of war, to send new raised regiments to serve in the islands of the West-Indies; and though the injudiciousness of the practice has long been discovered, it does not yet appear to be discontinued. During the late war there were several corps sent out to those

countries newly recruited, the consequence of which was, that though not a man died by the sword; yet in the short space of two years, there scarcely was a soldier left. A great part of this dreadful mortality undoubtedly arose from the climate; yet some share of it seems likewise to have proceeded from the particular circumstances of raw undisciplined troops.—Men newly enlisted in England, are generally of gross and full habits, and too often accustomed to irregular and dissipated modes of life. Under such circumstances, a sudden transition to a hotter air, joined with full meals, and the habitual indolence of a passage at sea, cannot fail to produce a plethoric state of the body, which is often rendered dangerous by the incautious use of strong liquors, or the ordinary exertions required in performing military exercises, under the influence of a powerful sun. I do not pretend to insinuate that those are the causes of remitting fever, but I am very sensible at the same time that they are causes which occasionally aggravate its danger, and which even sometimes accelerate its appearance. In soldiers who have been inured to a military life, such change of climate operates with diminished effect. The bulk of the fluids is perhaps diminished by a continuance of less full living; while the tone and elasticity of the moving powers are increased by habits of exercise and exertion.—The disposition to commit excesses is likewise repressed by the rigour of discipline; and the mind acquires a philosophical firmness from long service, which not only contributes to the preservation of health, but which enables the individual to sustain with fortitude the attack of diseases.

In passing from a cold to a hot climate, the first thing that occurs to be considered, is the effect produced by the simple increase of heat on the human frame. Expansion of the fluids, and consequent fullness of the vessels is constantly observed to take place

from such a change, frequently however accompanied with diminished energy of the moving powers, particularly where heat is combined with dampness of the air. To obviate therefore this natural effect of heat is the first general object to be attended to, in transporting troops to the tropical climates. The English, from the constitution of their bodies, and still more perhaps from their manner of living, suffer more from those sudden changes than some other European nations. The French and Spaniards are not only less gross constitutionally, but eat likewise less animal food, and drink their liquors greatly more diluted, than the natives of England. They do not probably owe more to medical assistance than the English; yet they are known to escape better from dangerous diseases; and their safety I might add has been remarked to bear some proportion to the different degrees of abstemiousness, which they are known to observe. An idea prevails with the generality of people, who visit warmer or more unhealthy climates, that it is necessary to eat and drink freely, as a security against the attacks of endemic fevers; but a very narrow observation will serve to shew, that good living, as it is called, has no such effects; and we may even soon perceive, unless blended by long established prejudices which flatter our appetites, that it actually is attended with pernicious consequences. The most abstemious, so far as I have observed, escaped the best, not only from the attacks, but particularly from the danger of diseases. With regard to the diet of a soldier, serving in a hot climate, I should be disposed to believe, that one spare meal of animal food would be perfectly sufficient in twenty-four^{hours}; and if it were easy to alter established customs, it would be most proper, perhaps, that it were made in the cool of the evening. Coffee, or tea for breakfast might likewise be substituted with advantage in place of the ordinary allowance of rum:

but this I must confess would be a dangerous experiment, Our soldiers have been so long accustomed to consider this gratuitous allowance of rum as their right, that no man could answer for the consequences of with-holding it. The practice certainly is pernicious, and the man, who first introduced it into the army, did no good service to his country. I do not deny that a judicious use of spirits might be of benefit occasionally: neither do I pretend to say, that, even the hardest drinking can be considered as a general cause of fevers; but it would not be difficult to produce evidence, that hard drinking aggravates the violence, and increases the danger of the disease, when it happens to take place; while I cannot perceive much reason for concluding, that the use of spirituous liquors has ever been productive of general good to the army, particularly in warm climates. But as I have just mentioned, that spirituous liquors have little claim to be considered among the number of those things, which contribute to the preservation of health: so I may add, with perfect confidence, that the allowance of rum granted to soldiers, has done much harm by ruining discipline, and good behaviour. If it is with-held for one day, discontent immediately begins to shew itself among the men. If with-held for any length of time, complaints sometimes rise to a state of mutiny, and desertions become numerous. But besides this, that soldiers seldom perform extra-duty with alacrity, unless they are bribed with a double allowance of liquor. A double allowance, drank undiluted, as is generally the case, is frequently sufficient to produce some degree of intoxication. I need not mention the disasters to which an intoxicated army is exposed. Disasters of a very serious nature have actually happened from this cause, and they might have happened oftener had the enemy been always vigilant, and bold enough to have seized the opportunity.

A deal might be said on the subject of abstemiousness. Moderation both in eating and drinking is essentially necessary to the health of troops newly arrived in hot climates; but a truth so obvious need not be enforced by many arguments. The example of the French and Spaniards afford a very convincing one. It is known to every medical person, that the fevers of hot climates are generally most dangerous in full and plethoric habits. It ought to be an object of attention therefore to obviate this cause of mortality, by means of spare living, and the cautious use of stimulating liquors: but soldiers have little self command, and seldom resist the gratification of their appetites. Hence it becomes the duty of their officers to enforce their compliance with what is proper, and to preclude them, as much as is possible, from the means of obtaining what is pernicious; but this requires great vigilance and attention, and often great severity. It is not enough that soldiers are obliged to eat in messes. The officers ought daily to inspect their meals, and inflict penalties where they observe transgressions. And further, as it is a matter of much importance to preserve troops in a state of health fit for action, and as the course of fevers is often uncommonly rapid in the West-Indies, it would be proper, perhaps, that the surgeon reviewed the men daily. The distant approach of the disease would be frequently discovered by this means, and the danger of it might probably be sometimes averted by timely assistance. Before men appear in the sick-reports, the fever is often considerably advanced in its progress.

Besides the alterations which might be made in the diet of troops, on their arrival in hot climates, some changes in the mode of cloathing might, perhaps, be likewise adopted for the sake of ease and convenience, if not for purposes of real use and economy. Round white hats would be the most proper covering for the head; and dowlas might be substituted with advan-

tage in room of the thick cloth, of which the coats of soldiers are usually made. There can be no grounds for supposing, that a soldier will not fight as well in dowlas as in scarlet; and there is certain proof that he will perform duties, which require exertion, with greater safety and effect, as the nature of his cloathing will preserve him cooler by some degrees. But though such alterations may be hinted, there is little room to believe that they will be attended to. In the present rage for military shew, it will be a difficult task to convince men to lay aside an uniform, which adds so much to the brilliancy of the appearance. Much stress seems at present to be laid upon the dress of the soldier, and I do not pretend to argue, that it is a matter of perfect indifference. It has certainly very often had visible effects upon the enemy; but these effects have oftener proceeded from a knowledge of the character of the troops who wore it, than from any thing formidable in the uniform itself. But to leave this subject of dress, I shall only observe, that a flannel or cotten wrapper would be more useful to a soldier, serving in the West-Indies, than a blanket; and perhaps the expence of it would not be much greater. It would serve for his covering in the night, and would secure him against the effects of cold, where occasions obliged him to go out.

I shall endeavour in the next place to point out some of those benefits, which may be derived to health, from habits of daily exercise. This is an object of the greatest importance, but unfortunately it is an object very little attended to in the British army. It appears, indeed, to be little regarded in most of the armies of modern Europe. I should incur a charge of presumption, perhaps of ignorance, did I attempt to point out the exercises which are the most proper for the forming of soldiers. Those only which contribute to the preservation of health, belong to this place. I may however remark, that the essential part of the

art of disciplining troops, consists in imparting sentiments of heroism and virtue to the minds of the men, in improving the exertions of their limbs, and in acquiring knowledge of the correspondence of their exertions when called into action. If I durst take so great a liberty, I should be inclined to say, that our ordinary exercises are flat and insipid in their nature; that they occasion no exertions, and excite no emulation: they neither improve the active powers of the body, nor inure the soldier to bear fatigue and hardship. The Romans, who owed more to the discipline of their armies than any nation on earth, were extremely rigorous and persevering in their exercises. They practised their soldiers in every species of service that might occur; so that nothing at any time happened with which they were unacquainted. Actual war was in reality a time of relaxation and amusement to the soldiers of this warlike people, who appear to have been trained for the service of the field, as horses are for hunting or the course. The Romans were not only sensible of the advantages which those habits of exercise procured them in action; but had also the penetration to discover, that they were eminently serviceable in the preservation of health. The words of Vegetius are remarkable. "*Rei militaris periti, plus quotidiana armorum exercitia ad sanitatem militum putaverunt prodesse, quam medicos.*" I made the same remark during the time that I attended a regiment in America, without knowing that it was supported by so great authority. I observed, when the men were in the field, sometimes even complaining of hardship and fatigue, that few were reported in the list of the sick: when removed to quarters, or encamped for any length of time in one place, the hospital was observed to fill rapidly. This observation was uniformly verified, as often as the experiment was repeated.

An idea has been long entertained, that the Euro-

European constitution cannot bear hard labour in the sun, or perform military exercises with safety, in the hot climates of the West-Indies. Hence a plan has been suggested, and in some degree I believe adopted, that regiments serving in those countries, be furnished with people of colour to do the drudgery of the soldiers. But this appears to be an innovation which ought to be admitted with extreme caution. It will evidently serve to increase sloth and idleness, and unless the persons of colour can perform the military duty in the field, their services will go but a short way in preserving the health of the troops. A soldier, notwithstanding he may have received the King's pay for twenty years or more, remains in some degree a tyro till his body has been inured to fatigue, and prepared to bear without danger the effects of the climate, in which he may be destined to serve. This is a part of the military discipline, indeed, no less necessary than a knowledge of the use of arms; and though it is a part of it, difficult to be accomplished, there is still room to believe, that it may be effected, even in the so much dreaded climate of Jamaica. It is a common opinion, that the fatigues of an active campaign in the West-Indies, would be fatal to the health of the troops; but the opinion has been assumed without fair trial. The exertions of a single day have often been hurtful. This was frequently the case in America, where the soldiers had remained for some time in a state of rest; but bad effects from the greatest exertions, in the hottest weather of summer, were extremely rare in that country, after the campaign had been continued for a few days. But that I may not seem to rest an opinion of so great importance on a bare analogy, I shall beg leave to observe, that young European planters undergo greater fatigues, and remain daily exposed for a longer time to the heat of the sun, than would fall to the lot of soldiers in the actual service of the

field. I might likewise further confirm the opinion, that an Englishman is capable of sustaining fatigue in the West-Indies, equally well with the African, or the native of the islands, by mentioning a journey which I once performed myself. I lived about four years in Jamaica, during the greatest part of which I believed that death, or dangerous sickness, would be the consequence of walking any distance on foot; but I afterwards learnt that this apprehension was vain. I left Savanna la Mar in the year 1778, with the design of going to America; but having embarked in a hurry, and forgot a material piece of business, I found a necessity of being put ashore, after having been two or three days at sea. I was landed at Port Morant, in St. Thomas's in the East, from which I went to Kingston by water, where learning that there was a vessel at Lucca, in the Western extremity of the island, nearly ready to sail for New-York, I set out directly, that I might not lose the opportunity of a passage. My finances not being in a condition to furnish horses, I left Kingston on foot, about twelve o'clock, and accomplished a journey before it was dark of eighteen miles. I did not find I was materially fatigued and still persisting in my resolution, travelled a hundred miles more in the space of the three following days. It may not be improper to remark, that I carried baggage with me, equal in weight to the common knapsack of a soldier. I do not know that so great a journey was ever performed on foot by an European, in any of the islands of the West-Indies; not so much I am convinced from inability, as from idea that such exertions are dangerous. But as it appears from the above fact, that the European constitution is capable of sustaining common military fatigues in the climate of Jamaica; so I may add that it ought to be a principal object of military discipline, that soldiers be practised with frequent marching, and the performance of

Other exercises of exertion, if it is actually meant that they should be useful in times of war. The fate of battles, I might observe, depends oftener on rapid movements, in which the activity of the limbs is concerned, than on the expert handling of arms, which is acquired by the practice of the manual. I observed formerly, that abstemiousness and temperance were among the best means of preserving health, or obviating the danger of the diseases to which troops are liable on their first arrival in hot climates; but the rules of temperance are little regarded by English soldiers at any time, and almost constantly transgressed wherever extraordinary labour is required of them. To such causes of excess, joined with the great heat of the sun, we may perhaps impute many of the bad effects of marching, or of moderate fatigue in the West-Indies. In the journey which I have just now mentioned, I probably owe my escape from sickness to temperance and spare living. I breakfasted on tea about ten in the morning, and made a meal of bread and salad, after I had taken up my lodging for the night. If I had occasion to drink through the day, water or lemonade was my beverage. In the year 1782, I walked between Edinburgh and London in eleven days and a half; and invariably observed, that I performed my journey with greater ease and pleasure, where I drank water, and only breakfasted and supped, than when I made three meals a day, and drank wine, ale, or porter. In the following summer I carried the experiment farther. During the months of July and August, I travelled in some of the hottest provinces of France. I generally walked from twenty-five to thirty miles a day, in a degree of heat less supportable than the common heat of Jamaica, without suffering any material inconvenience. I breakfasted about ten o'clock on tea, coffee or syrup of vinegar, made a slender meal of animal food in the evening, with a great proportion

of fallad and vegetables; but never drank the weakest wines without dilution. The great refreshment which I found from syrup of vinegar and water, convinces me, that the Romans had good cause for making vinegar such an essential article among the provisions of their armies.—The state of luxury and our depraved appetites, unfortunately do not suffer it to be adopted by the English. I ought perhaps to make an apology to the reader for introducing my own experience on the present occasion: but I must add, that I have only done it, because it enables me to speak from conviction, that an English soldier may be rendered capable of going through the severest military service in the hottest islands of the West-Indies, and that temperance will be one of the best means of enabling him to perform his duty with safety and effect.

I mentioned before, that the military exercise of the English army is ill calculated to excite a spirit of emulation among the men. It is in fact considered only as a piece of drudgery, in which there are few who have any ambition to excel. It has little effect in improving the activity of the limbs, or hardening the constitution of the body; so that it may better sustain hardship and fatigue. But feeble as its effects are in the view of increasing exertion, or preserving health, it is generally almost intirely discontinued when troops arrive in hot climates; a practice, which has arisen from a superficial and mistaken view of the subject. Sloth and indolence are the bane of a soldier in every climate; exercise and action are the greatest preservatives of discipline and of health. It would be reckoned presumption in me, and it does not belong to this place to point out those exercises which might be proper for the forming of soldiers. But every one knows that walking, running, wrestling, leaping, fencing and swimming, are often called into actual use in the practice of war. These are

such exercises likewise as excite emulation, and are practised with pleasure by the individual. They harden the body, increase the power of the limbs, and by furnishing the officer with a view of the different degrees of activity, may often enable him to place his men in the ranks, according to the uniformity of their exertions; a more useful mode of arrangement in time of action, than uniformity of exterior form. I may add in this place, that sea-bathing will be extremely useful in most cases, in increasing the vigour and preserving the health of soldiers serving in warm climates. There no doubt will occur many cases, in which it is improper; but in general it may be employed with great benefit. I chiefly impute it to this cause, that I did not experience a single day's indisposition, during the four years that I lived in Jamaica.

It has been known for many ages, that the cause of intermitting and remitting fevers, the most formidable diseases of hot climates, owes its origin to exhalations from swampy and moist grounds. It often happens likewise, that those low and swampy grounds are the most accessible parts of a coast, or that towns and settlements have been placed near them—to attack or defend which falls to the lot of the soldier. It not being therefore in the power of a military commander to remove the natural disadvantages, which I have mentioned; it is only in his power to shew his judgment and attention, by applying the best remedies to obviate their effects. It is certainly an object of the utmost consequence to preserve troops in a state of health fit for action: and no person will deny, that every care ought to be employed in choosing the best situations for quarters, or even temporary encampments, that the nature of the duty will permit. We learn from experience that fevers are little known in rough and hilly countries, where water flows with a rapid course; while we likewise

know, that they are common in low and champaign countries, where water stagnates, or has only a sluggish motion: independent of which, those situations which are in the neighbourhood of swamps, or near the oozy banks of large rivers, have always been observed to be particularly liable to such diseases. If therefore the circumstances of the service do not forbid, no room can be left to doubt about the propriety of stationing troops in the mountainous or hilly parts of a country; while I may likewise add, that where necessity confines them to the plain, the sea shore will in general be found to be the most eligible. But besides the above general character of local situations there are likewise some subordinate circumstances, which deserve to be particularly attended to in choosing the ground of encampments. It is very commonly believed that high and elevated situations are the most uniformly proper for this purpose; but this in fact is not, by any means, a general rule. A high and dry situation does not contain any thing hurtful in itself; but it is more than others exposed to the effluvia which are carried from a distance. It is the peculiar nature of exhalations to ascend as they proceed from their source; in confirmation of which truth I have had several opportunities of witnessing, that this cause of disease was carried to rising grounds in a state of great activity; while it apparently passed over the plain or vallies which lay contiguous, without producing any material effects. From the knowledge of this fact we are furnished with this obvious remark, that it will be proper to interpose woods or rising grounds to the progress of those noxious vapours; or where such natural advantages do not exist, it might be serviceable to burn a chain of fires in a temporary encampment, or even to raise a parapet wall to over top the barracks, where necessity requires a more permanent station.—It would be a matter of utility, could we determine with any cer-

tainty to what distance from its source, the noxious effluvia extend; but this is a question which we cannot hope to ascertain very exactly. It is not uniformly the same in all situations, depending on the concentrated state of the exhalation at its source, the obstacles it meets with in its progress, and the nature of the ground over which it passes, or to which it is directed. I have known its influence very remarkable at the distance of a mile and a half, on the top of a hill of very considerable elevation.

The conveniences of trade have often tempted colonists to place their towns on the banks of rivers, without regard to the healthfulness of the situation.--- The choice of such spots, injudicious as it evidently is, has been greatly approved of, and warmly recommended as preferable to others for the encampment of troops, by a very celebrated medical authority. Sir John Pringle considers the banks of large rivers as extremely proper for this purpose, on account of a free circulation of air; but I am sorry to observe, that Sir John Pringle's opinion on this occasion appears to have arisen from his theory, rather than that his theory has arisen from observation. We have actual experience of the unhealthfulness of the muddy banks of large rivers in hot climates; and we have little cause to dread diseases, which originate from confined air in America, the West-Indies, or perhaps in any country where troops are employed in the field.

I have just now observed, that the banks of large rivers, in the opinion of Sir John Pringle, afford the most eligible situation with respect to healthiness for the encampment of troops. I may add, that the same author has likewise recommended open grounds for this purpose, in preference to woods; and that the same favourite idea, viz. a free circulation of air, has influenced his advice. I will not contend, that open, dry and cultivated grounds may not be preferable to grounds covered with wood, where the heat

of the climate is moderate ; but I have no doubt in asserting, that encampments on lands, the woods of which have been newly cut down, as is generally the case in times of war, are of all others the most unhealthy. I have myself seen several examples of it. Perhaps it is in a great measure owing to this cause, that new countries are generally so fatal to the first settlers ; as also, that troops suffer so remarkably in carrying on the sieges of places which are surrounded by woods : it being constantly observed, that effluvia from moist lands, when first exposed to the action of a powerful sun, are always highly pernicious. The Romans, whose observations on subjects which relate to war, may be opposed with confidence to the authority of the most celebrated moderns, were fully sensible of the advantages of encamping under the shelter of wood. We learn from Vegetius, that their armies resorted to the cover of a wood, not less carefully, than that they avoided the vicinity of swamps or marshes. There are in reality various circumstances, which contribute to render such situations both healthy and agreeable. If troops are encamped in the body of a wood, the noxious effluvia, which may be carried by the winds from neighbouring swamps, are stopt in their progress ; the lofty shade of the trees preserves the air cool and more refreshing than the atmosphere of the open country ; while we know from experience, that moist and swampy lands do not send forth their noxious vapours, in any remarkable degree, unless where they are acted upon by the heat of a powerful sun.

I shall only further observe, with regard to the cause of intermitting and remitting fevers, that a space of time almost constantly intervenes between exposure to the noxious effluvia, and the subsequent appearance of the disease. It is not indeed uniformly the same in all cases, appearing to depend not only on the concentrated state in which the exhalation is

applied to the body; but on the general aptitude of the individual, and the various occasional or exciting causes, which facilitate or resist its operation. It was in a few instances only, that I saw the disease appear before the seventh day. It was oftener the fourteenth, twentieth, or even longer. Upon the whole I may remark, that septenary periods has a considerable power in influencing the time of its appearance.

Having offered a few observations in the preceding pages, on the diet, exercises and choice of the quarters or encampments for troops in hot climates; I shall now add a few hints respecting medical care and management. It will probably be supposed, that no attention with respect to this subject has been omitted. Regiments are provided with surgeons, and armies have always been furnished with ample hospital establishments. But this perhaps is not enough. It is necessary that the duties of these stations be well executed, as well as well designed. The office of surgeon to a regiment is an office of trust and of primary importance; the appointment to it, however, does not seem in general to be sufficiently attended to. The surgeoncies of regiments, till lately, were allowed to be bought and sold; in consequence of which abuse, little other qualification, came to be required, than the command of the purchase money. Thus it often happened, that young men, who had attended a course of anatomical lectures, or walked the rounds of an hospital for a few months, came at once to be entrusted with the care of the lives of six or seven hundred soldiers, who, as they are raised and maintained at a great expence, deserve, on the score of economy, independent of every other consideration, to be well taken care of. It would be superfluous to use any arguments to prove the prodigality of committing the care of a regiment to men, who have not had professional experience in any country, and who are totally unacquainted with the diseases of the countries

to which they are frequently sent. If we are disposed to believe that there is any thing in medical treatment, we can scarcely avoid making the conclusion, that many lives are lost from this cause. It must not be understood, that I mean any thing disrespectful to the surgeons of the army, by this insinuation. I know that a regiment is an excellent school for medical knowledge; and that the best practitioners have occasionally appeared in the army; but I wish strongly to inculcate the propriety of obliging candidates for this office, to produce evidence of their qualifications, before they are admitted to such an important trust. It is not enough, that a young man, who offers himself to take charge of the health of a regiment, should know to perform an operation with dexterity. Handling a knife in reality is the least part of a regimental surgeon's duty. The office of physician is his daily employment, to execute which properly, both years and experience are required. It certainly ought therefore to be an object of concern with those who are entrusted with the office of superintending the medical appointments of the army, that the candidates for surgeoncies be obliged to submit to such trials, as may in some degree afford proofs of their abilities. It would be a proper regulation, perhaps, that no man be permitted to propose himself for the surgeoncy of a regiment, before he has arrived at such an age, as may have furnished him with general experience; and further, that he give testimony of actual abilities by the treatment of diseases in an hospital, under the inspection of an able physician, to whom the duty will be prescribed to examine the mode of practice with rigour. A trial of this sort might be better trusted to than the commendatory letter of a professor; or even the diploma of Oxford or Edinburgh. There is not any thing chimerical in the proposal. Nothing in short is more practicable; but it is scarcely to be expected, that men of talents and education will give

themselves so much trouble, that they may be admitted into a service which holds out few advantages. The salary of regimental surgeons is small; and it is perhaps no paradox to say, that this is a cause of great expence to the nation. The bare subsistence, which the service affords, furnishes no inducement for men of abilities to enter, at least to remain any length of time in the army, which unfortunately has been considered in no other light, than as a place, where surgeons may pass their noviciate; but which they are generally disposed to leave, as soon as they are qualified to execute the duty properly. Medical knowledge is gained only by experience but independent of medical knowledge, an acquaintance with the habits, characters and dispositions of soldiers is a matter of so great importance, that old surgeons, even of inferior abilities as medical men, have generally been observed to have a proportionably small list of sick in their respective corps. The qualities that are principally required in a regimental surgeon, exclusive of medical learning and knowledge, are acuteness in discerning the characters and dispositions of men, and above all, boldness and decision in the application of remedies. Life is often lost in unhealthy climates, by the dilatoriness and timidity of common practice.

Having mentioned just now, that there appears to be a remissness in examining the qualifications that are requisite for the office of regimental surgeon, I might perhaps, with equal justice, extend the remark to the appointments in the general hospital.—The power of appointing physicians or surgeons in the hospital, has generally been lodged with the commanders in chief, and I might say, without transgressing the bounds of truth, that merit has not always been the best claim for promotion. It would be invidious to be more particular in censuring what is passed. It is only hoped, that the subject will be enquired into, and such remedies applied, as will pre-

clude similar abuses in future. The general hospital has ever been a heavy article in the expences of war; and if it were fair to form an opinion of the whole, from the part which I have seen, I should not hesitate in declaring, that the establishment is in a great measure superfluous. I have no doubt, in obtaining the suffrages of people of experience, that general hospitals are ruinous to military discipline; that they promote sloth and indolence, the worst disease to which a soldier is liable, and that they extinguish very speedily the ardour for the service of the field.-- There is in fact no exaggeration in the assertion, that the man, who has spent two or three months in a general hospital, is less of a soldier than when he was first recruited. It is only I may add by habits of exercise, even by toils and fatigue, that men at last attain the properties of good soldiers: while it is only by constant practice of such discipline, that they are preserved in a state fit for the performance of their duties. These active qualities are speedily extinguished by the habits of sloth and indolence, which prevail in general hospitals; but besides this, it is likewise certain, that cures are often there protracted to months, which might have been accomplished in the course of a few days, if circumstances would have permitted the men to remain with their regiments.— Regimental surgeons have many inducements to exert themselves in restoring their men speedily to health, which act only with feeble power on those who have the management of general hospitals. The former likewise possess some advantages, of which the latter are destitute. They know the habit and dispositions of the patient; they see the disease in its first beginnings, and are enabled to seize the most favourable moments for acting with decision. The above are considerations, which ought to make us backward in removing sick soldiers to general hospitals; I may add, that such is the nature of military diseases, that

there does not, perhaps occur one case in twenty, which might not be treated properly by the surgeon of the regiment, if attention, and a very little expence were bestowed in providing necessary accommodation. But besides that, the diseases of soldiers are seldom of such a kind, that they might not be treated properly by regimental surgeons, if government were at the expence of supplying a few conveniencies.

I may farther observe, that together with the indolence naturally attached to general hospitals, and uniformly hurtful to military discipline, there is often actual danger to life, by removing men in critical situations, or by the necessary intermission of medical assistance, where continual and vigorous exertions are required. The diseases of hot climates, particularly the fevers of the West-Indies, are often most acute and rapid in their course. The surgeon of a regiment perceives the approach of danger, and, sensible that his situation does not enable him to do justice to his patient, determines to remove him to the general hospital. But time is lost before this can be accomplished. It is seldom that any thing is done after it is deemed proper to send a sick man away; neither does it commonly happen, that any thing material is attempted on the day on which he is received. Thus one day at least, sometimes two are completely lost in cases, where every moment is of consequence. Time is precious in the fevers of hot climates; and the decision or neglect of an hour often determines the fate of a patient.

It is an observation, which cannot fail of having frequently occurred to people who have served any time in the army, that it would be a very great advantage to the service, if sick soldiers could always be taken care of by the respective surgeons of the regiment. I have endeavoured to shew, that the plan is practicable and easy; and I may further add, that the expence necessary for such an arrangement,

would not amount to one third of what is usually spent in general hospital establishments. If this idea were adopted, nothing more would be required, than that proper lodgings, proper provisions, and a sufficient supply of medicines, were furnished for the sick; that the surgeon of the regiment be well qualified for his station; and that an inspector be appointed for a certain portion of troops, to take care that the duty be well and diligently executed. By this means a general hospital, as far as regards medical treatment, might be abolished, or at least greatly abridged. Where fighting was expected, extra-surgical assistance would still be necessary. Such an inspection of regimental hospitals, as that I have mentioned, seems to be perfectly sufficient for the care of the health of an army, in ordinary occasions. The greatest precautions, however, ought to be taken, that the inspection does not degenerate into a nominal duty. The inspector ought to visit the different quarters, examine minutely into every article of the management of the hospitals, and order that general reports be published annually; and that some mark of approbation be bestowed upon those surgeons, who appear to have executed their duty with the greatest diligence and ability. It ought to be a concern of government, however, that no person be appointed to inspect regimental hospitals, who is not well acquainted with the diseases of the climate, in which the troops happen to serve.

N O T E S.

C H A P. I.

THOUGH I have described the endemic fever of Jamaica as distinctly as is in my power ; yet as I have often observed that we attain more accurate ideas from the detail of a particular history than from general description, I shall select from my notes two or three cases which may serve to give a clearer view of the different species of the disease. And in the first place I shall describe an instance of fever, which was distinguished through the whole of its course by symptoms of the general inflammatory diathesis.

(1) Lennox, a soldier of the 60th regiment, aged 40, of a firm and compact habit of body, was seized on the 3d of December, between eight and nine in the morning, with a slight horror or shivering, preceded and accompanied by other usual marks of fever. The symptoms of coldness and shivering went off in the course of eight or ten minutes. A hot fit succeeded, with a good deal of head-ach, hurried respiration, considerable thirst, a strong, full, and frequent pulse. After a continuance of four or five hours, sweat began to appear on the head and breast, which extending gradually to the extremities brought with it a tolerable distinct remission of the fever. 2. The fever appeared to be gone off very completely by ten o'clock at night. The patient rested well during

the night, and continued in this same state of ease till about five o'clock in the evening. He then became uneasy and restless, with head-ach and a slight feverish heat. 5. The feverish indisposition declined in the course of the night. He became easier towards morning, and about seven might be said to be in a state of remission. About nine a paroxysm commenced, similar to the paroxysm of the first day, though with a still slighter degree of horror and shivering; the hot fit ran still higher, with much head-ach, thirst, and a strong vigorous pulse. The sweating at last made its appearance, and the violence of the fever declined: there still however remained some degree of head-ach, pain of the back, and thirst, with an aversion to food, and a more than natural frequency of pulse. 6. Rested tolerably; but still is not free from head-ach and pain of the back: the tongue is dry and foul, and the coat which covers it is smooth, but of considerable thickness, and of a cream colour. About four in the evening the paroxysm of a fever made its appearance, similar, in some degree, to the paroxysm of the fourth, but of a much greater degree of violence. It continued for eight hours, and declined gradually towards morning. 7. There were no perceivable marks of fever at seven in the morning. A little after nine, however, a paroxysm commenced, similar, in every respect, to the paroxysm of the fifth. 8. About three in the afternoon a paroxysm began similar to the paroxysm of the sixth, but still more violent. It declined after the usual duration, and was succeeded on the ninth by another paroxysm similar to that of the seventh. The remission which succeeded appeared to be still more perfect than any of the preceding; the sweat was even more copious, and the pulse became softer and more expanded after it than it had hitherto done. 10. A paroxysm returned about half past two, similar to the paroxysm of the eighth, but not less violent in degree. It termi-

nated, however, in a more fluid and universal sweat; the pulse and the state of the skin returned perfectly to what they were in health; the mucous coat separated from the tongue; the eye and countenance assumed their natural serenity, and unequivocal marks of final crisis appeared on the morning of the eleventh. The above case is an instance of the double tertian; the fever of the even day terminated the disease; and the pulse through the whole course was vigorous and strong, or marks of inflammatory diathesis, in a moderate degree, were constantly present.

(2) Henley, a soldier of the 60th regiment, was seized on the 6th of May, about five in the evening, with a nausea, or unpleasant affection at stomach, marks of great languor and debility, a slight feeling of coldness and horror, a very weak and frequent pulse, head-ach, pain of the back, and other symptoms which are usual in the accession of fevers. After a continuance of ten or twelve hours, these symptoms were so far gone off, that the patient was considered to be in a state of remission. 7. The exacerbation of the fever returned again about the same hour in the evening at which it had first come on, though without marks of preceding coldness or shivering. The pulse was small, obscure, and extremely frequent; the heat of the body was not increased very materially; the thirst was only in a moderate degree, but there was much nausea, an aversion to food, a disposition to faint in an erect posture, deep and heavy sighing, tremor of the tongue, and a sad and desponding state of the eye and countenance. 8. The symptoms of fever abated towards morning, and a remission, though by no means a distinct one, took place. The pulse became somewhat slower and more expanded; the sighing and anxiety abated a little, and there was evidently a state of greater ease; though there still remained marks of great debility, and signs

of spasmodic stricture on the surface of the body. The heat was lower than it usually is in health. About five in the evening the symptoms, which had prevailed in the former paroxysms, returned again, but with considerable aggravation. The head was affected with delirium, and there was a considerable degree of tremor and starting. 9. Easier in the morning, though the remission was in no degree more complete than the former. About the usual hour in the evening the same symptoms returned with aggravation. 10. The remission as the former; the heat of the body below natural; the pulse obscure and frequent; the signs of debility very great. The exacerbation returned again at the usual hour; the paroxysm appeared to be somewhat more violent; the delirium was higher, the heat greater, and the pulse acquired rather more strength and fulness. 11. Easier in the morning, with a remission in every respect as complete as the former; the pulse distinct, and rather more expanded; and, upon the whole, an appearance of rather more vigour. The paroxysm was renewed in the evening as usual. 12. Remission in the morning rather more complete: more vigour in the pulse. The exacerbation as usual. 13. In the morning, instead of the usual remission, there appeared marks of a complete and final crisis; the sighing, which had been troublesome throughout the course of the disease, vanished; the eye and countenance assumed their usual serenity and cheerfulness; the pulse became slower, softer, and more expanded; and the tongue parted with its coat or covering. The above is an instance of fever with symptoms of nervous affection.

(3) Sergeant Negli, on the 2d of November, about eight in the morning, was seized with horror, shivering, and other Symptoms, which are usual in the accession of fevers. The hot fit did not run very

high, and before evening the paroxysm was considerably abated. 3. This patient is now in the state of remission, the heat of the body is not greater than natural; but the case seems to be attended with some symptoms which are not very common in the fevers of this country. The countenance is clouded, dark, and grim; the appearance of the eye is sad and desponding; and he expresses an uneasiness in his feelings which is not easily accounted for. 4. The paroxysm returned about four in the morning. It was greatly more violent than the preceding; and though it might be said to remit very completely, if we judge by the heat of the body and state of the pulse; yet there still remained some uncommon and unpleasant feelings. The eye and countenance were not only dark and desponding, but the tongue was covered with a slimy mucous coat, through which the red surface appeared obscurely; there were strange and unaccountable twitchings of the stomach and bowels, disturbed sleep, frightful dreams, and foreboding apprehensions. 5. A paroxysm came on this evening near twelve hours sooner than it was expected. After expressing an easiness at stomach, and throwing up some matter of a dark colour, he was suddenly seized with a stupor and insensibility, from which he could not be roused by all the applications of art. He died in about sixteen hours. This case affords an instance of fever with marks of a peculiar malignity. The appearances of danger were sudden and unexpected; and, as it was among the first instances of the kind which I had seen, I was disappointed, and in some degree confounded at the event.

(4) Thomson a young man aged twenty, after more than usual exercise in the heat of the sun, was seized with sickness, shiverings, and other signs of fever, about nine o'clock in the morning of the 3d. of February. The pulse was hard, frequent, and

irritated; the eye was sad, and sometimes glistening; the countenance flushed, but rather dark and overcast; the respiration hurried; nausea was troublesome, with a good deal of anxiety and restlessness. The paroxysm continued long, and did not indeed go off very perfectly at last. 4. Rested but indifferently; is now somewhat easier, though the remission is far from being perfect; the thirst is considerable; the tongue dry and foul, the stomach loathes all sorts of food; and he seems to be much distressed with flatus and ructus; the stools are dark-coloured and foetid; the pulse is more frequent than natural, hard and irritated, and the skin is only partially moist. 5. An exacerbation of fever happened about nine in the morning. The symptoms were of the same kind as in the first paroxysm, only somewhat more violent in degree. The anxiety at stomach was particularly distressing, and there appeared still more evident marks of putrescent tendency in the alimentary canal. 6. An uneasy night: an imperfect and obscure remission: the gums redder than they naturally are: the eye has a glistening appearance, and the countenance is still confused and clouded: the tongue is dry; the thirst great; and ructus and flatus are very distressing: the pulse still irritated and quick: there is not any very remarkable disposition to faint in an erect posture: the stools foetid. 7. The exacerbation returned about the same hour as on the fifth, and with still greater aggravation: the symptoms of distress in the stomach and bowels were particularly alarming; with nausea, nidorose belchings, and large watery foetid stools. 8. Somewhat easier in the morning, though the remission can only be said to be obscure. 9. The exacerbation happened at the same hour as on the seventh, and continued for nearly the same length of time. 10. Instead of obscure remission, marks of final crisis are now evident; the pulse is returned nearly to its natural state; the eye and

countenance have assumed their usual serenity; the skin is moist, and gives no marks of remaining spasmodic stricture; the anxiety and ructus have ceased; and the state of the stomach and bowels appears to be almost natural. The above is an instance of fever, in which there were very evident signs of putrescent tendency in the alimentary canal; even some obscure marks of its progress in the general system, complicated, however, with an irritated state of the vascular system, or such symptoms as might be considered as belonging to the apparent inflammatory diathesis.

(6) Cunningham, a sailor, aged twenty-five, was seized on the 5th of July, about five in the evening, with sickness, shiverings, head-ach, and the other usual signs of the remitting fever of the country. Its more distressing symptoms were nausea and vomiting. 6. The remission is tolerably distinct; but there is still a good deal of head-ach, thirst, and signs of debility; the tongue is dry, and the pulse is more frequent than natural. The paroxysm returned about five in the evening with increased violence, accompanied with severe retching, and copious vomiting of bilious matter. 7. Better in the morning; the vomiting has ceased, and the remission is tolerably distinct. The exacerbation returned at the usual hour, with the same distinguishing symptoms of copious bilious discharges. 8. Remission in the morning as usual; the exacerbation in the evening as the preceding, with distressing and severe vomiting. 9. The usual remission in the morning. The paroxysm likewise recurred in the evening about the usual time, but not with the usual symptoms. Instead of vomiting of bilious matters, there was some degree of delirium, tremors, startings, and other symptoms of nervous affection. 10. These symptoms remitted in the morning, but there still

remained signs of great irritability and weakness. The same train of symptoms returned again in the evening: the delirium and tremors were still in a higher degree; the pulse was small and frequent; and there was occasionally a great disposition to faint in an erect posture. 11. Better in the morning, though there are not yet any marks of crisis. The exacerbation returned again in the evening, with symptoms similar to those of the preceding paroxysm. 12. Remission in the morning similar to the former. Exacerbation in the evening rather more violent. 13. Remission as the former; the pulse however appears to be rather fuller than it has been since this change happened in the circumstances of the disease. The paroxysm returned at the usual hour still more violent, though with greater marks of vascular excitement. 14. Evident marks of crisis: the tongue begins to part with its covering; the eye and countenance appear clear and animated; the pulse is slower and fuller; and the state of the skin does not give any indication of existing spasmodic stricture. This case presents an instance of fever, the first part of the course of which was distinguished by uncommon bilious discharges during the time of the paroxysms; the latter part of it by affection of the nervous system.

C H A P. II.

1. As I mentioned before that we attain more accurate ideas from the detail of particular cases than from general histories; I therefore relate the method of cure, which was pursued in those examples which are described in the sixth chapter.

1. *Lennox.*—On the 4th of December; or second day of the disease, the solution of salts with a small portion of emetic tartar was given by a wine glass full at a time, till it operated plentifully. 5. Some powders of nitre and camphire. 6. Two scruples of bark were given every two hours during the remission, with an injunction that the nitrous powders be repeated during the time of the paroxysm. 11. The above plan was persisted in till marks of crisis appeared. Not more than one ounce of bark was given during all the remissions.

2. *Henley.* 7. The usual solution of salts was given, but without any addition of emetic tartar. It operated plentifully. 8. The bark was begun this morning, with injunctions that it be administered every two hours during the remissions. 9. A blister was applied to the back of the head and neck, with a bolus of camphire, opium, and valerian. Wine was ordered, together with the bark, as soon as the remission should begin to appear. This plan was persisted in till the crisis arrived, which was on the 13th.

3. *Negli.* The patient was purged on the 3d with the usual solution of salts, to which was added so considerable a portion of emetic tartar, that it likewise operated by vomit. 4. Bark was given in the

usual quantity, and at the usual intervals. 5. As soon as the fever came on, blisters were applied to the head, and likewise to the legs; but they produced no perceivable effect. The patient died, and probably fell a sacrifice to the disease, from my not having early enough perceived the malignity of its nature.

4. *Thompson.* 4th, The solution of salts with emetic tartar was administered in the present case as it had been done in the others. It operated plentifully, but had no material effect upon the disease. 5. Saline draughts in the state of effervescence were given frequently. Bark and wine were ordered in the remissions, with as much lemonade as the patient chose to drink. 9. Glysters of cold water, impregnated with fixed air, were employed two or three times with apparent benefit. 10. The bark, wine, and saline draughts were given liberally, yet notwithstanding, the disease seemed to complete its natural course.

5. *Cunningham.*—6. The nausea and vomiting were so distressing in the first paroxysm, that, in compliance with the patient's earnest entreaties, I consented to give an emetic. 7. The symptoms were aggravated, and the emetic was repeated but without advantage. 9. Anodynes were given during the paroxysm with saline draughts in the act of effervescence. They moderated the vomiting but did not entirely remove it. Blisters were applied to the head and legs; bark and wine were given in considerable quantity; but the disease continued till the 14th without material alteration.

C H A P. III.

I. As the cold bathing, which I have so strongly recommended in the cure of fevers, has an exterior appearance of being a rash and hazardous remedy, I shall relate some cases which may enable the reader to judge more precisely of its real effects. Cold-bathing I may remark, appears to have been occasionally employed by the Greek and Roman physicians, after the time of the Emperor Augustus; but I was only a young man when I went out to the West-Indies, and cannot pretend to say that I was acquainted with the writings of those physicians, or that I possessed much knowledge of diseases, except the little that could be retained from a cursory hearing of university lectures. The first hints of this practice were therefore accidental, and arose from a conversation I had with the master of the vessel, in which I went passenger. This person commanded a transport in the war 1756, and was present at the siege of the Havannah. As he was talking one day of the state of the fleet, he mentioned accidentally, that some men were sent aboard of his ship ill of fevers; several of whom, he observed, jumped into the sea during the delirium which attended the paroxysms of the disease. Some of them, as might be expected, were drowned; but the most part of those who were recovered from the waves appeared to be greatly benefited by the ducking. The fact, which, from the veracity of the man, I thought I could depend upon, struck me strongly, and I resolved, in my own mind, to bring it to the test of experiment as soon as an opportunity should offer: neither was it long after my arrival in Jamaica, that I had occasion to visit a sailor whose situation seemed to justify such a trial. The poor

man was aboard of a ship, which lay at anchor about a mile from the shore. He had been ill two days; the delirium ran high; his eyes were red and inflamed; his respiration was hurried; he was anxious and restless in a high degree, whilst together with those marks of excitement, he was occasionally languid and disposed to faint. His skin being dirty furnished an ostensible excuse for trying this remedy. But it was previously thought proper to draw some blood from the arm; which being done, some buckets of salt water were dashed on the shoulders. He was now laid in bed: a copious sweat ensued, succeeded by a distinct remission, and a total change in the nature of the symptoms. The success I met with in this instance was more than I had expected; I was therefore encouraged to try the same mode of bathing in a person who came under my care some weeks thereafter, and who had been ill of a fever six or seven days. This patient had been bled and blistered;—emetics and cathartics had been likewise employed, and bark had been given in the usual manner, for the three last days. The fever, however, had now in a great measure lost its type. The man was low and languid; his eyes were dim; his vision indistinct;—his pulse was small and frequent, and, when the head was raised from the pillow, not to be felt. Though it did not appear that he could reasonably be expected to live long, I still wished to get him conveyed to the deck, that a trial might be made of the effects of cold bathing; but the situation was so ticklish, that I felt some uneasiness in setting about it. At last he was lifted through the hatch-way in a blanket, though I must confess that I was not without apprehensions that he might die under my hands. Some wine was then poured down his throat; and he was sprinkled with cold salt water, as he lay on the deck. Appearing to be somewhat invigorated by this process, he was raised up very gently, and several buckets of

the sea-water were dashed about his head and shoulders. He was then laid in bed; the pulse soon became large and full. I left him in a copious sweat, and was agreeably surprised next day to find him sitting on the deck, to which he had walked on his own feet. I shall only mention another instance of the good effects of cold bathing in the fevers of the West-Indies, which is perhaps more decisive than either of the former. A boy, aged fourteen, had been ill of a fever seven or eight days. Nothing had been omitted, in point of treatment, which is usual to be done in similar cases. Bark and wine had been carried as far as could be serviceable, or even safe; yet death seemed to be approaching fast. The success of cold bathing, in some instances similar to the present, so far exceeded my expectation that I was induced to make trial of it in the case before me, though I was not altogether without apprehensions that death might be the consequence of the attempt. The business, however, was accomplished without accident; and next day the boy was able, not only to sit up in bed, but even to walk over the floor. After instances so unequivocal as the above, it would be superfluous to mention any others. I shall only add, that I have tried the remedy, in various situations, always with safety, generally with astonishing success; so that I cannot forbear recommending it even at an early period, in the fevers of the West-Indies. It communicates tone and vigour to the powers of life, and diminishes irritability in a degree far superior to all other cordials or sedatives. The bathing was managed in the following manner: the water, which was required to be of a refreshing degree of coolness, was generally dashed by means of a bucket on the head and shoulders. It was likewise found that its good effects were heightened, in some cases, by previous bleeding, and by the previous use of warm bathing. This may seem a rash practice to those who argue

without experience ; but, setting aside the authority of the ancients, we find it confirmed by the example of a person who was not a physician, and who, therefore may be supposed to be less under the influence of a favourite opinion from which he might be led to disguise the truth. Busbequius, who was sent on an embassy to Soliman the Great, was obliged to travel to Amasia, where the Sultan then sojourned. In his return home he was seized with a continued fever, and very severely harrassed by it. The disease gained so much ground during the journey, that he found it necessary to stop at Constantinople to attend to the recovery of his health. The practice which was adopted to effect this may appear to be singular, and by many, perhaps, will be thought to be hazardous and rash. He mentions, that, after enjoying the luxury of warm bathing, he was suddenly sprinkled with cold water. His words are, "*Idem, scilicet, Quaelbenus me a balneo exeuntem frigida perfundebat, quæ res etsi erat molesta, tamen magnopere juvabat.*" (*Iter. Constant. p.*) His physician, Quaelbenus, who seems to have been a man of excellent judgment and careful observation, had probably learnt the practice in his travels in Asia, as it does not appear to have been commonly known in Europe at that time.

C H A P. IV.

1. But besides these testimonies of physicians, in favour of the practice of drenching with cold water, the memoirs of Baron Trenck furnish us with a curious and very convincing proof of the efficacy of this remedy, in extinguishing, almost like a charm, the violence of a burning fever. The Baron, when ill of a fever in the prison at Madgeborough, unfortunately broke the pitcher which contained his daily allowance of water. The fever was violent, and he suffered the most inexpressible torments of thirst, for the space of twenty-four hours. When the usual supply was brought to him next day, he seized the pitcher with eagerness, and drank the water with such avidity, and in so great quantity as is scarcely credible. The consequence was a total removal of the disease. To this I might add an instance, which happened to myself at Savanna in Georgia, in the year 1779. In the excessive hot weather of the month of July, I was seized with the endemic of the country, in a more violent degree than was commonly seen. In the third paroxysm of the disease, my desire for cold water was ravenous. A pitcher of it was drawn from the pump, which I drank off instantly without the least abatement of the thirst. The draught was repeated in a few minutes, in quantity not less than a quart. The thirst was effectually quenched, and the fever seemed to vanish. But though the fever appeared to be extinguished as it were by a charm; yet the stomach and hypochondria became distended, yellowness of the eye and countenance succeeded, with a considerable degree of debility which remained for two or three days. I must, however, remark with regard to this case, that the effects were not the same as they have been usually reported to be by

authors. The fever was extinguished ; but neither vomiting, sweat, or any other sensible evacuation ensued. The ancients, I may further observe, seem to have administered cold drink only in the advanced state of fever, when signs of coction began to appear ; in which case, it is impossible to form a certain opinion of its precise success. That cold water may be employed with effect, it is necessary that the thirst be intense, perhaps that it be purposely provoked, and that it be fully and completely satiated. If managed in this manner, it probably will not often fail of extinguishing the fever ; yet I must not omit to mention, that unless it is managed with a great deal of caution and judgment, it may also often irrecoverably extinguish the powers of life.

2. In support of this opinion, I shall mention a case, which fell under my own observation about a twelvemonth ago. I was called to a young man, a sailor, ill of a fever of a very dangerous and alarming kind. It was the eighth day of the disease before I saw him. He had not been hitherto in the least benefited by any thing that was tried ; neither did any remedy which I could think of, though employed with almost desperate boldness, in any degree check the progress of the disease. The power of speech was lost, and even swallowing was performed with difficulty ; the eye was languid, nay almost without motion ; the countenance was ghastly ; and many livid spots, some of them nearly the size of a six-pence, made their appearance on different parts of the body. I proposed bathing, and the friends of the young man, considering the situation as desperate, consented that I should make a trial of it ; more, perhaps, to comply with my desire, than from expectation of any benefit that might result from it. But in setting about it, it unfortunately happened, that an utensil proper for the purpose could not be procured, so that

we were compelled to be contented with a general fomentation. This was applied in as complete a manner as circumstances would permit. A blanket was soaked in warm salt water, and the body was wrapped in it from head to foot. In a short time the skin became soft and warm, sweat began to flow; the eye and countenance began to resume their animation, which had been almost extinguished, the pulse rose; swallowing was performed with less difficulty; and next day the colour of the spots was evidently brighter. So far the change was favourable; but a regular supply of wine and cordials having been neglected during the following night, the pulse sunk, and things returned nearly to their former situation. The fomentation was again repeated, in consequence of which the extremities and surface of the skin became warm and moist, an effect which was no sooner produced, than the blanket was suddenly removed, and the face and breast, particularly, were sprinkled with cold water, in which a large portion of salt was dissolved. The cold had the effect to cause the patient to shrink at the first, yet in a short time he appeared to be refreshed very remarkably. The powers of life grew gradually stronger; though the marks of crisis were not very evident for several days. To the above I might add some other instances, where effects were similar; but I avoid swelling the notes to too great extent, by entering into particular details. I shall therefore only observe in general, that cold bathing was usually of service. It imparted general tone and vigour to the powers of life, and by increasing the activity of the vascular system, probably sometimes rendered the crisis complete, where it naturally would not have been so; but I cannot venture to say that I ever carried it so far that the disease could be said to be precipitately extinguished by it.

N O T E S

TO THE

CHAPTER UPON YELLOW FEVER.

(a) **I**N compliance with the language of medical authors, I have described the following disease under the name of Yellow Fever, though I am perfectly sensible, that the appellation is not by any means proper. There are some instances of the disease perhaps where yellowness does not at all appear, and in no one does it ordinarily shew itself till the latter stages. I know also that most of the practitioners of Jamaica consider it only as an aggravated species of the remittent; the common endemic of hot climates. It appeared to me I must confess in a different light; but I shall attempt to describe the two diseases accurately, and leave it to the reader to judge for himself. It may not however be improper in this place to take notice of the opinion of Dr. Moseley. Dr. Moseley has lately written a treatise on this disease, and endeavoured to persuade us that it is no other than the *καυσος*, or ardent fever of the ancients. But the yellow fever of the West-Indies, by Dr. Moseley's own confession, is in some manner peculiar to strangers newly arrived in tropical climates. The *καυσος* we are informed, made its appearance in the islands of the Archipelago, and on the coasts of the contiguous continents indiscriminately among men and women,

natives or foreigners : in fact it has not, as far as I can perceive, any claim to be considered as a distinct species of disease. If I rightly understand the spirit of Hippocrates, or the description of the still more accurate Aretæus, *καυσος* in reality is only an accidental condition of the common endemic of the country, where the force of the fever is chiefly exerted upon the stomach and alimentary canal. In this manner it appears frequently in Jamaica, and in the southern provinces of America. In the hot months of summer, it appears occasionally in every climate : and is not necessarily accompanied with, nor does it depend upon a general inflammatory diathesis of the system for its existence.

6. Authors seem generally to have attributed the black colour of the vomitings observed in this disease to blood effused into the cavity of the stomach ; but the falsity of this opinion is sufficiently proved by the appearances which are observed on dissection.

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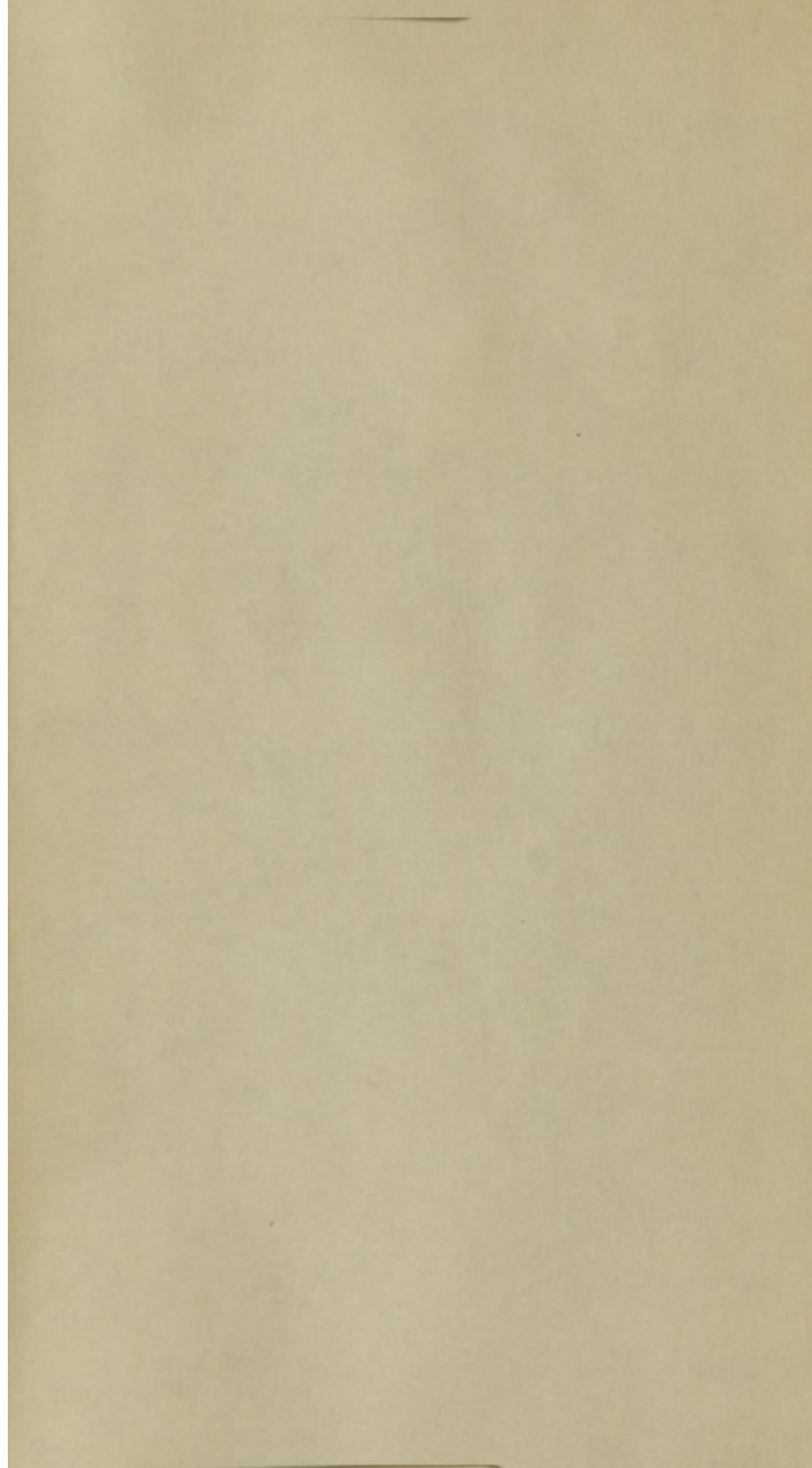
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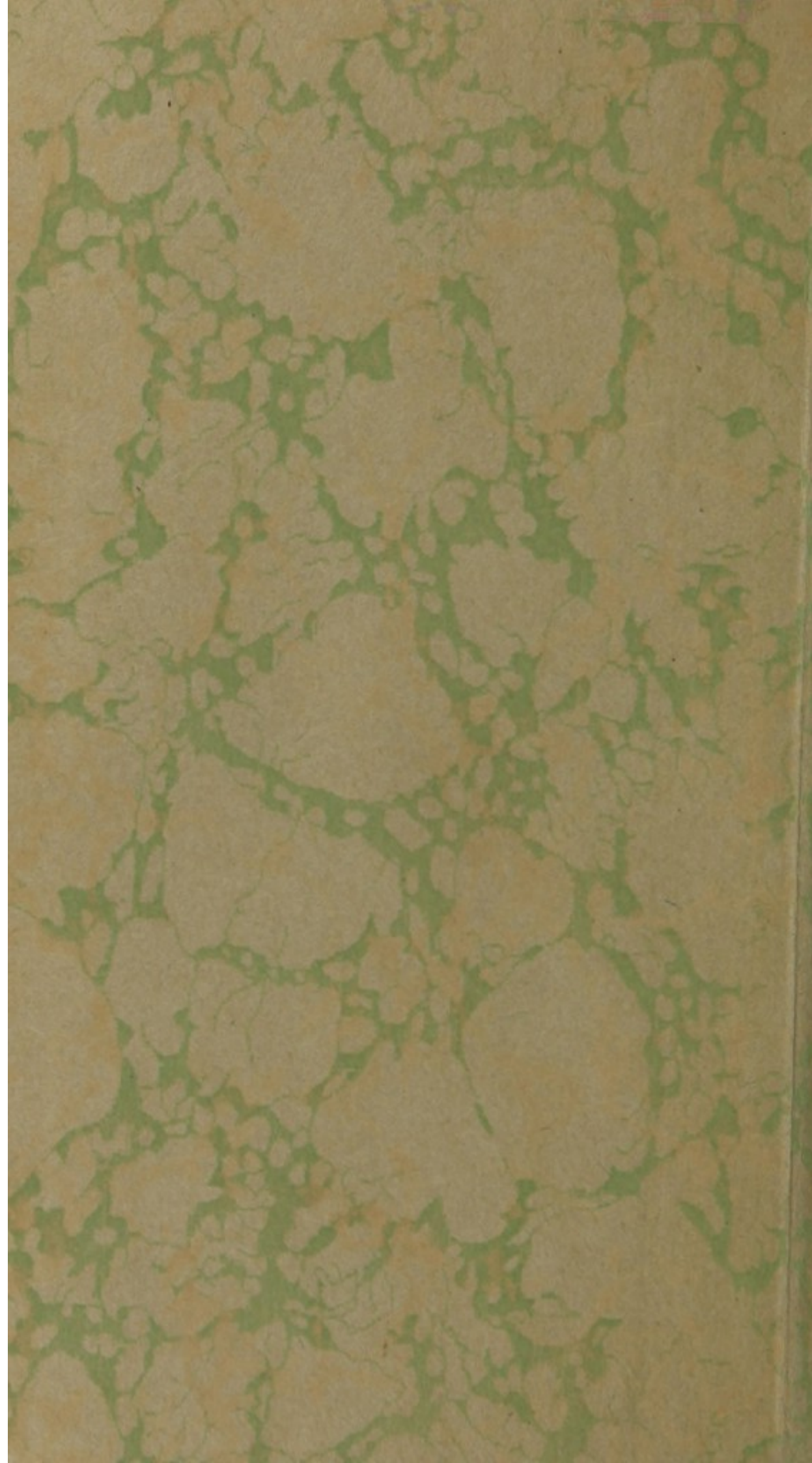
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